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ART. I.—*On the Etiology and Diagnosis of Smallpox and Chicken-pox.* By W. L. SUTTON, M.D., of Georgetown, Ky.

I am perfectly aware that the subject of this paper is one which by a large proportion of the profession will be deemed particularly dry and uninteresting. This is by no means to be wondered at, when we reflect that a very respectable number of physicians in the west have never seen a case of smallpox, and very reasonably suppose they never will. When we reflect, however, that although smallpox is rare, very rare, yet it occasionally makes its appearance very unexpectedly; that physicians who have never before seen it, sometimes fail to recognise it until its nature is pointed out by some non-professional person who has been more or less familiar with it; that, as has happened within my own knowledge, a respectable physician of fifteen or twenty years' experience should attend three successive well marked cases

of the disease, (having his attention repeatedly directed to smallpox, but having no clue to its origin), without being able to identify it; that during this state of uncertainty immense injury would be done by the spread of the disease; and when to this it is added, that, after considerable research into the opinions of our standard writers on the etiology of smallpox and chickenpox, and reflections on the subject, I think that the more closely we examine these opinions, the less we will be satisfied with the precision of the ideas therein advanced; we shall, I apprehend, consider it as a subject of some importance, and worthy of some reflection on the part of those whose duty it may very suddenly become, to guard a community against a loathsome, horrible, and dangerous disease, or on the other hand, to quiet the alarm which may be needlessly raised by the cry of "*smallpox.*"

Under this impression I propose to lay before the profession a few facts observed by myself during the brush of smallpox, which afflicted our town in 1846, with some reflections which those facts suggested.

On the 28th June, 1846, I visited Joseph, a negro boy, æt. 8 years, unprotected against smallpox, who had become indisposed the previous day. I found him complaining much of pain in the head and abdomen, referred to the umbilical region; considerable fever; tongue somewhat red and covered with a white fur. ℞ Calomel grs. x., tart. antim. and potass grs. ½.

29th. Much as yesterday; rather less fever; medicine vomited and purged him several times.

30th. Still some fever, but less than on the 28th. After my visit yesterday, an eruption appeared upon the forearms. To-day it is upon the body and legs. It is very copious, and consists of clear watery vesicles, prominent (conical); viewed perpendicularly, the centre is transparent, but approaching the circumference of the base, it is less perfectly so.

July 1st. Eruption full upon the face; same character. At the base the vesicles are about the size of the head of a diaper pin, very uniform as to size, and almost as thickly set as is possible for them to be and remain distinct. Indeed on the left wrist, for a space something less than a quarter of a dollar, they were confluent. I do not think there was any other instance in which two were in contact.

3d. There is evidently a redness at the base of the vesicles, which are now becoming milky in their appearance; no swelling of the face or extremities.

4th. Vesicles more prominent for two or three days; fever has disappeared.

5th. Begins to scab upon the face; vesicles upon the body and extremities begin to shrivel. No swelling of face or extremities; no fetor.

7th. Desquamation upon the face and body nearly complete: going on freely on the arms, less so on the legs. Scabs small, thin, black; one of them presented under the microscope a slight depression in the centre.

These scabs I examined with a microscope, and compared them with some obtained from a case of confluent smallpox. Their structure seemed exceedingly different. I subsequently compared them with scabs obtained from a case of mild smallpox, the patient being of nearly the same age. There was not much difference in appearance or thickness as viewed with the naked eye; with a magnifying glass of about four diameters, there did appear to me to be some difference, but I had no microscope to make a correct comparison.

For two or three weeks after desquamation was completed, Joseph presented a very ashy face, but after five or six weeks, his face became sleek and entirely free from all vestiges of the eruption. In contrast with this, I will state that every negro who had smallpox, how mild soever may have been the attack, has yet (May 1848,) dark colored spots upon the face.

When this case occurred, we had smallpox and varioloid in four houses in town, viz:

1st. Several in the house of F. F. Johnson, which is situated on Main street, directly north of the house in which Joseph lived, at the distance of 390 feet (the depth of a square and a street), with a range of buildings between. The rooms in Mr. Johnson's house are all spacious. The house itself three rooms deep, the patients generally in the back and middle rooms. Some two or three, however, were in the attic, which from the size of the house affords very fine rooms. In this attic are two windows facing the south. About this time, it was said that the peculiar odor of small pox was perceptible in the evening for a short distance up and down the street.

2d. Two or three in a house some 150 or 200 feet still farther north, and directly in the rear of Mr. Johnson's house.

3d. Two cases of varioloid at Dr. Barlow's, on the same street, and same side of the street as Mr. Johnson.

4th. One case of varioloid at Captain Shepard's on the same street, two squares off.

Chickenpox had been in town for six or eight months.

Joseph was not known to have had any chance to contract smallpox from the time it was understood to be in town, viz: June 1st; certainly not before. It is true that he had been on Main street twice in that time, it is said only twice. At each time he must have come within from 120 to 150 feet of Mr. Johnson's house; and the house to which he was sent, was next to Dr. Barlow's. The doctor and his daughter (the two cases in that house) had occupied the back rooms of the house, and the foot pavement in front of his house had been obstructed. Dr. Barlow had broken out with varioloid on the 31st of May, after two or three days of severe fever; his daughter a few days after. No intercourse between Joseph and any of Dr. Barlow's family was known, nor was any likely to have taken place.

On the 30th of June, when I saw the eruption, I immediately sought the committee whom the Trustees had empowered to act in such cases, and informed them that I feared we had another case of smallpox; adding, that the only source of doubt was the fact that the eruption had appeared upon the arms, body, and legs instead of the face, and was itself too much developed for the stage of the disease. By their direction, I took Dr. Craig to see the case in the afternoon, at which time it looked less like smallpox than in the morning, and Dr. Craig concurred with me in opinion, that it would not prove to be smallpox. From this time until the vesicles became milky, there was little resemblance to smallpox. On the 4th of July, a person seeing the case for the first time, and knowing that smallpox was in town, would most likely have considered it as an unequivocal case of that disease. There was, however, at no time any one pock seen with umbilicated or even flattened top; neither did they become hemispherical, but remained conical until they began to shrivel; neither had they the perfect purulent appearance of smallpox. On the 5th of July, this patient, together with a brother and sister, was vaccinated; four or five days afterwards, his vaccination, as well as that of his brother, seemed to have been successful; but from sheer wantonness, he scratched the forming pustule from his own and his brother's arm. Subsequently both were revaccinated. His brother's took; his did not; whether any foul play was used, is not known.

In reflecting upon this case, it occurred to me that Joseph's mother might have been vaccinated whilst pregnant with him —she assured me, however, that such was not the case.

Polly Ann, sister to Joseph, and occupying the same room, had been complaining for several days, when, July 5th, she took a dose of calomel and ipecac., and was vaccinated.

9th. Still complaining, with some fever, the vaccination appearing to be successful. ℞ Calomel grs. viii., to be followed with vermifuge.

Evening. The forehead is thickly covered with an eruption resembling measles or flea-bites.

10th. Eruption upon the face is very evident; it has also appeared on the arms and legs, but less thickly; entirely distinct; of some size. Between the ring and middle finger, on left hand, is one vesicle perfectly identical with those which appeared on Joseph; all others want the conical form and are decidedly turbid in color.

11th. Various pustules have a decidedly umbilicated appearance, and the child was ordered to the hospital, where she passed through a regular course of confluent smallpox.

18th. She appeared to be doing well; the swelling of the face which had been considerable was subsiding. The pock upon the face had begun to subside, and there was very considerable fetor; had sufficient appetite, which seemed to be on the increase. Two days afterwards, she died rather suddenly, apparently from congestion of the lungs.

During the summer—indeed about the time of these occurrences—a negro man, belonging to David Emison, living four miles from town, had an attack very much like that of Joseph, except that he had been vaccinated, and after two days of slight indisposition, sickened, whilst at work in a field, and commenced breaking out immediately. The eruption was very copious and conical; not umbilicated. His wife had just recovered from chickenpox. One of Mr. Emison's daughters was convalescing from varioloid. This negro had not been in the room where the young lady lay, but had been in an adjoining room for a short time. I did not see this man, but the above facts I learned from Dr. Emison, who was living in the family.

The question which I propose is, was Joseph's case one of smallpox? It is an easy matter to answer this question dogmatically, in the affirmative or in the negative, but it may not be so easy to convince others of the propriety of that answer.

Our standard authors teach that it is an easy matter to dis-

tinguish a well marked case of smallpox from any other disease; that nothing but ignorance or gross negligence can confound them. But this, if variola, was clearly an irregular form. It is admitted, I believe, that variola may be so mild that its nature may be doubtful; but ought there to be any difficulty in discriminating a case as severe as this was; one, too, in which the eruption was so copious? But it is said that variola cannot be confounded with anything but chickenpox. I shall purposely avoid the term varicella as the synonym of chickenpox, for reasons which will be sufficiently evident as we proceed.

As I presume there is no doubt that Joseph's case was either smallpox or chickenpox, it will be proper to look into their distinctive features, and see whether there is any way to determine positively as to their diagnosis.

The first point which I shall examine is as to the identity of the cause.

Dr. John Thomson, (whose work upon varioloid, etc., I have entirely failed to procure, although I have taken much pains in the search), appears by common consent to have done more to enforce the belief that smallpox and chickenpox are dependent upon the same specific contagion, than any other person. He has three cardinal points, which, if demonstrated to be true, certainly would go far to settle the question. These points, however, are not only debatable, but have been proved to be erroneous to the satisfaction of many physicians. Although I cannot hope to examine these points in a manner more satisfactory than other men have done, yet inasmuch as every man views objects in a light more or less peculiar to himself, and as it is necessary to have the same thing presented to our minds repeatedly, I shall examine the subject somewhat in detail. The points are,

1. That smallpox and varicella reciprocally produce each other.

2. That smallpox and varicella always coexist.

3. That varicella never develops itself except in persons who have either had smallpox or been vaccinated.*

It is always a matter of some importance, that in a controversy the parties should distinctly understand each other. It is important, too, that the point of dispute should be kept distinctly before the mind. It seems to me that much of the difficulty in this matter has grown out of a neglect of this caution.

I do not know that I can illustrate this remark better than by quoting a passage from Rayer, which was intended to put the truth of the first of the propositions just mentioned beyond controversy. "It is," says he, "not only during the epidemic prevalence of variolous disease, that varicella or chickenpox has been observed to give rise to variola; but under other circumstances which render the fact more conclusive and altogether beyond reply. No case of smallpox had occurred in this town (Kirriemuir) for nine years, till last winter; when an idle boy, who was in the habit of wandering about the country, happened to be at a house where some of the family were said to have smallpox. He himself had been vaccinated some years before. On his return home he was seized with febrile symptoms, and confined two or three days to his bed, when an eruption similar to chickenpox made its appearance. Immediately the fever abated, and in a few days more he left his bed and attended a cattle market half a mile distant from the town, without any bad consequences. About a week afterwards, one of his master's children was taken ill, and went through the regular course of smallpox in a mild manner; then a second similarly; a third suffered in a very alarming degree from the confluent kind; a fourth worse than the two first; and the youngest, eight months old, had what, if the other cases had not occurred, I should without hesitation have called chickenpox,

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* Cazenave on Cutaneous Diseases, p. 74. Rayer's great work on Diseases of the Skin, pp. 147, 148, Eberle on Children, p. 419.

for there was little or no fever, the pustules were filled with a watery fluid, which was not converted into the purulent appearance of smallpox. None of these children had undergone vaccination."*

This case proves several things:

1st. That a person who has been vaccinated, by being exposed to variolous contagion may have that disease in a modified form. It is now known that the eruption in cases of varioloid may vary in appearance from a slight papular eruption, to one differing but little, if at all, from that characteristic of regular smallpox.

2d. That contagion arising from varioloid produces, in those who are unprotected, regular smallpox.

3d. That contagion emanating from the same source may give rise to a very mild, a moderate, a very severe, or a fatal case of smallpox; but it certainly does not prove that smallpox gives rise to chickenpox, or that chickenpox gives rise to smallpox: unless, indeed, it be previously proved that chickenpox and varioloid are identical,—the very point in dispute. No one disputes that varioloid is of variolous origin.

I presume that few persons would come to any other conclusion than that the boy above mentioned took, by exposure to smallpox contagion, the varioloid disease; and that he communicated the smallpox to his master's family because they had not been protected by previous smallpox or vaccination, precisely as is known to be the legitimate consequences of such exposures.

As to the nature of the last case mentioned, we are left entirely in the dark. It may have been an exceedingly mild case of smallpox; or it may have been genuine chickenpox, altogether uninfluenced in its appearance by the existence of smallpox in the family; or it may have been some vesicular

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* Letter of Mr. John Mulloch, surgeon, to Dr. John Thomson, in his account of the Varioloid Epidemic. 8vo., London, 1820.

disease entirely unconnected with either; for it is well known that all persons exposed to the contagion of smallpox, do not contract the disease. Further; we have no means of knowing whether this child was subsequently exposed to variolous contagion, and consequently what the result of such an exposure might be.

The reporter of the case has assumed that chickenpox and varioloid are identical; has given this as a case of chickenpox being produced by variola, and in its turn producing the original disease. Nothing but confusion and uncertainty can be the consequence of such looseness in language, and illogical reasoning. To prove a negative is certainly a difficult matter at all times. The most that can be done is to prove an *alibi*.

When the second proposition announces that chickenpox never prevails without the presence of smallpox, it is met by the declaration of Möhl, (*De Varioloidibus et Varicellis*,) "that from the year 1809 to 1823, the chickenpox was annually observed at Copenhagen without concomitant smallpox, and that both diseases have since prevailed, at intervals, epidemically, but always under circumstances which satisfied the physicians of the town, that their sources were distinct."

Large cities are perhaps not the most favorable situations for determining the precise cause of disease in any particular case. When a contagious disease is present in such a location, many may be unconsciously exposed to contagion, and ascribe the appearance of disease to very erroneous causes. Hence a person known to have been in contact with chickenpox, but not known to have been exposed to variolous contagion, may have smallpox, which nevertheless was of variolous origin. But when smallpox is not present, and chickenpox prevails for a series of years, as just quoted, the matter becomes, if not "conclusive and altogether beyond reply," at least highly probable. I have been living in Georgetown, a village containing some 1800 inhabitants, for fourteen years. During that time, chickenpox has been more or less prevalent at three or four different periods. No case of

smallpox had been known in town for about forty-five or fifty years, until 1846. We have no village containing more than two or three hundred inhabitants, nearer than twelve miles. In such a location the course of a contagious disease could be more easily and satisfactorily traced than in a populous city. During the fourteen years mentioned, there had been two visits of smallpox in the county at a distance from town of some eight and twelve miles, respectively, and at intervals of several years. I consider this last fact as of no importance, and give it only that I may state all the facts upon the subject.

I apprehend that if the experience of the physicians of our towns and country, west of the mountains, could be brought to light, we should have thousands of cases equally strong as that quoted upon the authority of Möhl, or that given upon my own. There are hundreds, perhaps thousands, of physicians in the Valley of the Mississippi, who have never seen a case of smallpox, whilst there are few who have not seen, what at least was called, chickenpox. How is it then that chickenpox never prevails without concomitant smallpox?

What proof Dr. Thomson adduces that chickenpox always appears in persons who had previously had smallpox or cowpock, I do not know. It seems clear that even those agree that chickenpox and smallpox have a common origin, are by no means agreed upon this point. Thus, Dr. Eberle (on Children, p. 422), says: "We may introduce here the statement of Reil, that variola occurring after varicella is much milder than when this disease has not been experienced, more especially if the varicellous affection has been severe. So that the occurrence of varicella would seem to shield the constitution in a partial degree from the violence of variolous contagion." I presume that the main support of these last two propositions (the concurrence of chicken and smallpox, and the universal precedence of variola or vaccine) has been given by the confounding it with varioloid or chickenpox. What Dr. T. alleges of chickenpox is acknowledged on all

hands to be true of varioloid; the error, if error there be, consists in extending the conclusions to chickenpox.

In the little brush of variola which we had here in 1846, there were two cases directly in point upon this third proposition. In one family two persons, who, about six months previously, had what the attending physician called very decided cases of chickenpox, were seized with smallpox. One had an attack of medium severity, and recovered; the other sunk under the disease. Neither of these persons had been vaccinated. Thus it would seem that, on the one hand, persons who have never had either cowpock or smallpox may have chickenpox; and on the other, that a person may have chickenpox, and afterwards die of variola.

Another fact is thought to prove the identity of the cause of the two diseases; (why not consider the diseases themselves as identical?) viz: cases occasionally occur in which it is difficult to discriminate—in which one physician would call the disease variola, and another chickenpox. This argument is specious rather than solid. If recognized as of force, we should be compelled to consider many diseases as identical, which, by universal consent, are held to be different. To say nothing of the varioloid, cases of which vary so exceedingly as to the appearance of the eruption that one may be considered measles, and another smallpox in its regular form, we know that pneumonia may so much resemble hepatitis, that it is extremely difficult to form a correct diagnosis. Cases of hoopingcough are sometimes so mild, that great doubts exist as to its being anything more than influenza. But who proposes to consider hoopingcough and influenza as being the same disease, or depending upon the same cause, or reciprocally producing each other?

*Character of the eruption.*—In variola, this begins with scarlet or red papulæ, presenting to the touch a sensible resistance, but not much raised. These papulæ becoming more and more distinctly defined and elevated, and after a day or two converted into, or rather surmounted by, vesi-

cles of whitish serum, rather elevated in the centre, but seemingly divided on the surface by cross lines, giving something of a cellular appearance when viewed through a magnifying glass. Within twenty-four or forty-eight hours, this cellular appearance is lost, and a depression appears in the centre, the pustule increasing in size, and the contained fluid becoming more opake. From the sixth to the ninth day, the central depression is lost, and the pustules become hemispherical. About the tenth or twelfth day, small brown specks appear on the summit of the pustules, which again assume a somewhat flattened appearance. From this time the pock subsides by the bursting of the cuticle and exudation of the contained pus, or by the absorption of the latter. *

This is the usual course; but the eruption sometimes, especially in children, goes through its course in a time considerably shorter than that mentioned. Sometimes it is much longer.

In chickenpox, small red elevations, irregularly circular, and having in their centre a small transparent vesicle, make their appearance on the first day. These vesicles augment in size for two or three days. Some are acuminated, whilst others are flat. On the second or third day, the fluid which they contain, from being transparent, becomes milky. The fourth day a red areola surrounds some of them. Towards the fifth desiccation commences, and on the sixth they are replaced by small brown scales. These small thin crusts dry from the circumference to the centre, and drop off on the ninth or tenth day. They are void of the umbilicated appearance. †

* See Rayer, p. 150, also note by Bell on the next page. Armstrong's Lect., p. 487. Wilson on Febrile Diseases, vol. i., p. 426. Evanson and Maunsell on Children, p. 237. Cullen, First Lines, vol. i., p. 215. Cazenave, p. 140, etc., etc.

† Cazenave, p. 76. Evanson and Maunsen, p. 238. Wilson, vol. i., p. 427. Armstrong, p. 487. Cullen, vol. i., p. 232. Wood, vol. i., p. 288, etc., etc.

*Anatomical character.*—In variola, the pustule is found to occupy the true skin, which becomes reddened and sometimes ulcerated. Upon the surface of this, and beneath the cuticle, is a disk of pseudo-membranous matter, which, in the earlier stages of the eruption, adheres to the inner surface of the cuticle, and less firmly to the corion beneath it. The color of this disk is dull white, and its consistence rather friable. At a more advanced stage, a serous or purulent fluid is observed in small vacuities, or a widening cavity between this adventitious product and the surface of the true skin, and when the pustule is mature, a layer of pus separates it also from the cuticle.* After desquamation the skin is left pitted or red, either or both of which continue for many months, or during life.

In chickenpox, the vesicle is formed between the true skin and the cuticle; there is nothing of this pseudo-membranous disk; and the skin, after the disease, remains slightly purpuraceous, but soon regains its natural smoothness and color.

*Part on which eruption first appears.*—In variola, this almost always commences on the forehead and face, spreading thence to the neck, trunk, and extremities. In chickenpox, it very generally appears upon the body or limbs.

*Duration.*—It will be seen that variola usually requires from fifteen to twenty-five days for its completion; sometimes very many more. Chickenpox requires from six to twelve days.

*Number of pustules or vesicles.*—In smallpox there may be only fifteen or twenty, or they may be innumerable. In chickenpox, Wilson says, the eruption is never confluent or very numerous. The greatest number which Dr. Heberden saw was about twelve on the face and two hundred on the rest of the body.† On the other hand, Dr. Gregory says,

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*Wood, vol. i., p. 366. See also Rayer, and Cazenave.

† Op. cit., vol. i., p. 427.

when the eruption is very copious, the body has the appearance of having been exposed to a momentary shower of boiling water, each drop of which had occasioned a minute blister.*

Let us now examine Joseph's case. As to the cause of disease, it is very much a matter of conjecture. Smallpox and chickenpox were both in town; but he had not been exposed to either, so far as could be ascertained, except his two visits to Main street. It would seem that he had not approached those laboring under the smallpox sufficiently near to endanger himself. It must be admitted, however, that we are not yet certain at what distance the disease can be communicated. I am not aware of any case where the disease has been communicated at so great a distance as one hundred and twenty feet. Add to this that the patients at Mr. Johnson's were in the back part of the building, and in the second and third stories. The danger or probability of contracting the disease at Dr. Barlow's, I consider as almost nothing. The subsequent appearance of variola in his sister may be thought to prove that this case was itself of the same character. That, however, is a *non sequitur*. The character of Joseph's case was in nowise altered by the disease of Polly Ann. If it was smallpox before, it remained so; and *vice versa*.

It may be asked where did the latter get the disease? The somewhat natural answer would be, from Joseph. Where did Joseph contract it? The answer must be, we know not. Polly Ann most likely did not go abroad to get it, but there may have been communications between the servants of the two families, which were not known to their masters or the medical attendants, and thus the contagion may have been introduced.

We must bear in mind that "opinion is not settled as to the period of the disease at which it is contagious; some be-

* Watson, p. 676.

lieving it is so only after the commencement of suppuration," (Wood vol. i., p. 367). We must recollect too the period of incubation, which is usually ten to twenty days, according to Rayer, or nine or ten days, to a fortnight, according to Watson; or fourteen days, according to Evanson and Maunsell; or six to twenty days, according to Cazenave. If it should be decided that smallpox is contagious only after suppuration has commenced, then Polly Ann could not have received her disease from Joseph. Very generally, however, physicians believe that the disease can be contracted from a patient as soon as the eruptive fever has become perfectly developed. We will say, then, that Joseph infected Polly Ann on the day upon which I was first called, viz: June 28; if the eruptive fever commenced on the 5th July, the period when I first prescribed for her, we have a term of seven days for incubation,—the shortest time allowed by any one except Cazenave; and absolutely the shortest time allotted for inoculated cases. So far as I at present remember I do not know that the period of incubation is really shorter or longer in inoculated cases, or those in which the disease is taken in the natural way; but it would seem to be the general opinion that it is shorter in inoculated cases.

But as the eruption did not appear until the 9th, it is probable that the eruptive fever did not actually set in until the 6th. As I visited her brother on the 7th, I certainly saw her likewise, but I did not at any time consider her fever severe, or her case at all serious, until the eruption on the forehead made its appearance on the 9th.

The appearance of the eruption instantly excited my suspicion. As I remarked at the time, the vesicles seemed to me to be too large and transparent, and too pointed for variola. In the evening, when Dr. Craig saw him with me, they had grown in size so much that I at once gave up the idea of variola. If he had been vaccinated previously I might have considered it varioloid, as I do not know any limits to irregularity as to the appearance of the eruption in that disease. I

noticed the case very particularly, and if there had been a single umbilicated pock, I must have observed it. The vesicles when largest were about the size of the head of a diaper pin, perhaps something larger, gradually and uniformly decreasing in size until near the apex, when they rounded off. They were small at the base and more elevated than the smallpox pustule, and for several days entirely free from that milky, turbid appearance, which the latter so soon assume. There seemed no difference in size, color, form, or number, as to different locations. It will be remembered that Polly Ann had one of these pocks on her hand which contrasted strongly with those of smallpox in its usual appearance, being at the commencement much larger than the rest, and perfectly transparent: whilst they were somewhat milky at the first notice of a fluid in them.

The duration of the eruption was shorter than is usual with smallpox. It is to be noted, however, that in a little girl of about his age, the smallpox ran its course regularly in very nearly the same length of time, not perhaps exceeding two days longer. The crusts which separated from these two cases, (Joseph and the girl last mentioned), did not seem to differ much when viewed with a magnifying glass. There was, however, a very marked difference in the condition of the skin in the two. In a month, Joseph's ashiness had disappeared, and the skin was smooth and not colored. The girl yet wears the discolored spots upon her face. The number of pocks was perhaps not materially different in the two cases; both were very thickly sprinkled. There was no swelling of the face, hands, or feet towards the decline of the eruption (which took place very rapidly), corresponding with what is usual in variola. Neither was there any fetor. The fever in Joseph's case was considerable; but the epigastric distress was not great, unless he failed to designate the point of pain, which he referred to the umbilical region. He vomited, it is true, two or three times after taking the dose of calomel and tartar.

Take the whole history of the case, and it appears to me that we have a very fair sample of "conical varicella," as described by Cazenave, with something more of fever and distress than usually attends that disease.

Shall the fact that the sister had variola subsequently change our opinion as to the nature of the disease, and induce us to call it variola?

I make no comment on the case which occurred at Mr. Emison's, because the patient had been vaccinated, and, as before remarked, there are no limits to the modifications of the eruption of varioloid. It is fair, however, to say, that Dr. Emison considered the case one of chickenpox, until the vesicles became milky.

May 1848.

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ART. II.—*Cases of Disease.* By Dr. M. H. FEE, of Heltonsville, Indiana.

*Epilepsy.*—J. M., a young man, 18 years of age, having been subject to semi-monthly attacks of epilepsy from childhood, for which he had been treated by many skillful physicians, as well as a host of notorious quacks, without deriving any benefit from either, was, during the month of September, 1845, taken with intermittent fever, I think, of the quotidian type, for which he adopted no medical treatment, and which, in a few days, terminated in congestive fever. His bowels were very costive. The night previous to his seeking medical aid of me, his fever being excessively high, his epilepsy came on him, and during the night he had twenty very severe convulsions. His parents and friends had despaired of his recovery.

The evening of the following day, his father came and informed me of his condition, stating that he did not think it possi-

ble for him to survive till morning. I sent him forty grains of calomel with directions to take it immediately, and ten hours after, if he survived, to solicit its operation with salt and water enemata. I also sent three ten grain doses of quinine, directing to give the first dose one hour after the calomel; three hours after, another; and so on till all were taken. I remarked to the father, that I thought the quinine would suspend the epilepsy till the fever could be broken up. Next morning at 10 o'clock the father of the young man came to announce the good tidings. He said the quinine had not only suspended the convulsons, but that it had subdued the fever. The calomel, aided by the stimulating enemata, had moved his bowels freely, bringing away large quantities of scybala. His fever was effectually broken, and his convalescence was rapid. He has not had a fit of epilepsy since.

It is almost absolutely certain that quinine cured this case of epilepsy. I deferred reporting the case in order that time might test the matter. Thirty-one months have since elapsed, and no return of the disease has been experienced. Certainly sixty-two periods of exemption must be deemed a sufficient test.

*Inflammation and Abrasion of the Mucous Membrane of the Jejunum; Autopsy.*—J. N., on the 6th of April, called on me for advice in behalf of a child fourteen months old, cutting superior bicuspides. Bowels slow to act; discharges dark green, and small, mixed largely with mucus. Had, about six weeks before, an eruption of the skin, which disappeared rather suddenly; since which time it had been feverish at night. Skin constantly dry and harsh. Directed three doses of calomel to be given, one every alternate night, followed in the morning with oil sufficient to move the bowels mildly. In the interim, to give magnesia mixture to correct acidity.

On the 13th, called to visit the child. Treatment had not

relieved the symptoms; calomel had not perceptibly improved the secretions, nor arrested the morbid action of the bowels on which the mucous discharges depended. The diathesis was decidedly scrofulous; indicated by thin, blue nails, with crisp edges, and irregular; bluish cast of sclerotic coat of the eyes; languor of countenance; delicate prolabia; veins seen ramifying distinctly under the skin. An indolent boil on the left parietal bone attracted my attention, which, when opened, discharged a grumous matter, somewhat granulated. Directed a powder three times per day, composed of calomel one grain, chalk one and a half grains, Dover's powder half grain. Also spirits of nitre when fever was present. This treatment was continued till the 16th, at which time I visited the child. Powders had had no good effect, except to change the color of the discharge. Gums considerably swollen; cut down to the teeth as I had done at my other visit. Ordered the bowels to be freely moved with manna, molasses, and water. Spirits of nitre continued when feverish, with diluent teas. Quinine to be given in the absence of fever.

Did not visit the child again till the 19th. No improvement, but I think rather worse. Much inclined to sleep, with sudden startings; fever high, and bowels rather hot. Directed cold, wet cloths to the head, with warm fomentations to the bowels; warm pediluvia with an occasional dose of spirits of nitre. Left eight grains of calomel, mixed with one-eighth grain of morphine, to be given when fever cooled, followed in ten hours by castor oil. Quinine to be given next morning hourly, in the absence of fever.

20th—Afternoon. Child no better. The calomel and oil had produced but one very scant fecal discharge, fully two-thirds shreddy mucus. Moved the bowels once with warm water and lard enema. Mucus still abundant. Applied mustard to the bowels and legs, and put it on the use of diaphoretics and mucilage of gum arabic.

21st. Fever cool, but pulse 170; mucus still alarmingly

abundant; eye natural in appearance; convulsions occasionally of the spinal variety; no change in the pupil during the convulsions. Directed mucilage of flaxseed with spirits turpentine for enemata; spirits of nitre and paregoric every two hours; mucilage of gum arabic to be continued. Occasionally sudden grasping with the hand before the eyes.

22d. Worse; frequent strong spinal convulsions, sometimes passing to the brain, and distorting the face and eyes. Even during the cerebral part of the convulsion there was no unnatural contraction nor expansion of the pupil. No injection of eyes, but superabundant moisture. Had retained the enemata, and consequently no movement of bowels. Bowels moved with cold water and spirits turpentine. Nothing passed but shreddy mucus, tinged with yellow bile. More quiet after passage; pulse too frequent and hard. Put into a warm bath, medicated with spirits nitre dulc., for five minutes. Effected a reduction of fever and pulse. Directed mucilage and diaphoretics to be continued, with occasional enemata of flaxseed mucilage, hog's lard, and spirits of turpentine.

23d. Very bad; convulsions severe, often running to the brain, but the eye still normal in appearance, even at the time of the cerebral convulsion. Bowels not moved since yesterday; moved them with the syringe; passed nothing but mucus, slightly tinged with yellow bile. No abdominal tenderness, but a little more fulness in the umbilical region. Applied a large blister over the bowels. Directed the mucilage to be used per anum, as it had ceased swallowing, mixed with spirits nitre, sometimes with turpentine.

6 o'clock, P.M. Blister had risen tolerably well; skin inclined to moisture, but feeling too stiff and doughy. Cerebro-spinal convulsions occurring frequently; pulse 180, and small. As an experiment, gave an enema composed of four ounces water, three grains argent. nitrat., one drachm sulphuric ether. This was given at 10 o'clock, A.M. The child was perfectly quiet till half past 2 P.M., at which time

it had a very severe cerebro-spinal convulsion, and died at twenty minutes before 3 o'clock in the morning of the 24th.

*Autopsy, eight hours after death; abdomen only examined.—Appearances.* Stomach empty, firmly contracted, and strictly normal in structure and appearance. Duodenum normal till within about ten lines of the mouth of the ductus choledochus, at which point commenced a yellowish red discoloration, which continued downward about twelve lines below the mouth of the duct. Mucous membrane of the duct considerably injected and thickened. Jejunum containing nothing but flatus. In its course several discolorations were observed; some livid, some dark yellow, some lighter yellow, and some a shade darker than the healthy parts. The darker parts had lost the mucous membrane entirely; the lighter ones but partially. The last third of the small bowel was perfectly healthy. Colon healthy throughout its entire length. Rectum slightly injected, supposed to be caused by the stimulating enemata. Liver healthy in structure and appearance, some incipient tubercles were discovered on its surface; but none in an active state. Cystus felli filled almost to bursting with a viscid dark green bile; so much distended was it, that it stood out bold to view, fully eight lines beyond the surface of the liver. Urinary apparatus perfectly healthy. Bladder full of transparent urine. Arteries empty. Lower part of sympathetic nerve darker than natural. Not the slightest trace of peritonitis. About eight ounces of bloody serum in abdominal cavity. Peyer's glands presented no enlargement, or other change differing from a healthy condition. Many of the mesenteric glands, particularly in the second row, were enlarged, so as to lose their lenticular shape, had become globular, darker than natural, and were in an incipient state of softening. Spleen and pancreas were strictly healthy.

*Remarks.*—1st. This case differs from both dysentery and common mucous diarrhea, in which the mucous membrane of the colon is always implicated.

2d. We have in this case a specimen of the most insidious progress of an acute inflammation of the intestinal membrane, without the usual diagnostic symptoms; there not being during the whole course of the disease any abdominal tenderness, although the site of the first inflammatory action was in a coil of the intestine which had nothing to separate it from the anterior peritoneum, except the omentum majus, and certainly, from contiguity, by pressure, should have revealed diseased action. The pupil retained to the last its impressibility and natural appearance.

3d. Notwithstanding the eye constantly maintained its natural appearance, I have good ground for the belief, that lucid spectra flitted swiftly, occasionally, before its eyes at such times as it made a sudden grasp with its hand. If I should see such symptoms again, I would have no hesitation in referring them to abrasion of the mucous membrane of the jejunum, or upper part of the small intestine, and would treat it accordingly.

4th. I could not ascribe any good effects to a solitary article used;—all, alike, were worse than useless. Were I to meet again with a similar case, I would give the nitrate of silver in liberal doses. No milder remedy would change the morbid action of the mucous membrane.

5th. Was scrofula at the bottom of the disease in this case? The father has had tuberculous disease of the lungs for several years; and the mother has the characteristic marks.

6th. The convulsions were, with two or three exceptions, of spinal origin, caused by sympathy with the bowels. Those two which terminated in partial cerebral convulsions were conveyed to the brain through the medium of the sympathetic nerve. This was conclusively proved by the gradual diminishing of the heart's power for some seconds before the cerebral convulsions came on; and by the entire cessation of pulsation in this organ during the existence of the cerebral part of the convulsion.

7th. It may be easily perceived that medicated enemata could do no good in this case, for the simple reason that the disease was above the point usually reached by this class of remedies.

May 3d, 1848.

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ART. III.—*A Case of Anchylosis of the Lower Jaw relieved by an Operation.* By R. G. BUCKINGHAM, M.D., of Lexington, Lafayette county, Missouri.

I am induced to present the facts of the following case for publication, because it has been denied, by very high authority, that an operation for unlocking the jaw, closed by contraction of the zygomaticus major and minor, and other muscles of the jaw or face, produced by salivation, can be productive of any permanently beneficial results. As a matter of course, so long as this opinion prevails, the operation will not be attempted, and many cases where much good might result from its performance, will be left without an attempt to remedy them.

Having been called upon more than a year since, by Daniel H. Brown, to examine his son, who, by the unfortunate administration of calomel by his attending physician, became salivated to a considerable extent, I found the following condition of things: The jaws firmly closed, as nearly as could be ascertained, by extensive sloughing of the fibres of the zygomaticus major and minor muscles, and a portion of the anterior edge of the masseter, on both sides of the face; the muscles becoming very much shortened by the formation of large cicatrices during the healing process. The teeth were so nearly approximated that the smallest instrument could scarcely be insinuated between them; several of the molars on both sides, came away during the ulcerative process, and this afforded some room for the introduction of his food. He

had lost all use of the lower jaw except a slight lateral motion, not however sufficient for any degree of mastication; his food consisted almost entirely of liquids; very frequently in vomiting he would become almost suffocated from being unable to clear his mouth with his fingers as rapidly as it was forced from his stomach. His general health was declining rapidly, and the object of his father was merely to obtain my opinion in regard to the propriety of visiting the eastern cities, with a view to obtaining relief for his son.

Having made a careful investigation, and learning that the boy could masticate his food, and in fact had free use of his jaw after sloughing had ceased entirely, I determined upon an operation, having first obtained the consent of the little patient. Upon introducing the finger into the corner of the mouth, and reaching back as far as the angle of the jaw, I could discover distinctly the firm and rigid muscular fibres, which seemed to me to be the only obstacle to the opening of the jaw.

In the first place I expected, by dividing these muscles freely, to be able to open the mouth to as great an extent as was desired; and secondly, by an instrument, presently to be described, to keep it in that position until the divided parts were healed, so as to prevent the mouth from closing the second time.

Everything being in readiness, the boy was seated in a chair of convenient height, and, by the aid of an assistant, held firmly in his seat. I introduced a very small bladed scalpel into the mouth, and reaching the point as far back as the anterior edge of the masseter muscle, made a free incision through a portion of the former, and the zygomaticus major and minor muscles, dividing the latter freely upon both sides. The effect was just what was anticipated; the jaw relaxed seven-eighths of an inch; the instrument, after the hemorrhage had ceased, was introduced, and, in all, one inch and a quarter gained. This instrument, or a block of the proper size, was kept in the mouth until the parts were entirely healed.

More than a year has elapsed since the operation was performed, and the boy has as perfect use of his jaw in mastication as he enjoyed previous to his salivation.

The instrument to which I have referred, and the invention of which I claim, is made of iron, as light as possible, so that it retains sufficient strength, consisting of two jaws, similar to a cabinet-maker's hand screw, with two screws, the first passing through both jaws, the second through but one, the point resting in a socket against the other.

June 1st, 1848.

# REVIEWS.

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Art. IV.—*The Ancient World; or Picturesque Sketches of Creation.* By D. T. Ansted, M.A., F.R.S., F.G.S.; Professor of Geology in King's College, London, etc., etc. Philadelphia: Lea & Blanchard. 12mo. pp. 382. 1847.

*First Impressions of England and its People.* By Hugh Miller, Author of the "Old Red Sandstone," etc. "Do you not think a man may be the wiser (I had almost said the better) for going a hundred or two of miles."—*Gray's Letters.* London: John Johnstone. 1847. 12mo. pp. 407.

Geology has such relations to medicine, that we trust the propriety of introducing an occasional article into our Journal upon that subject will generally be conceded. Not a few persons we know there are, even in educated society, who believe that the world was created just as it is, and that, "for anything that appears to the contrary, it might have been made yesterday." To such, there can be but little interest in geology, its successive strata, and their organic remains. For them, there is nothing in the inquiries into a former condition of existence, calculated "to attract the fancy and excite the imagination." But there is another class to whom the evidence is clear, that ages before the creation of Man this world on which we dwell existed as the habitation of living beings, different from those now tenanting its surface, or inhabiting its seas; and for these, there is a deep interest in this history of antiquity. The proof of this is every-

where at hand, and requires only that the very commonest appearances of nature should be studied. As remarked by Mr. Ansted,

"No one can visit a chalk or sand pit, or look for one moment at a stone quarry,—no one can consider the appearance so commonly presented by a sea-cliff, a cutting for a road, or a sinking for a well, without being perfectly convinced that the limetstones, sandsones, and clays of which a great part of the earth's crust is made up, were not thrown confusedly together, but are arranged in some degree of order, lying upon one another in regular beds or strata. A very little inquiry into the way in which these strata rest upon one another, is amply sufficient to prove that order does exist in the arrangement of the greater proportion of the materials of the earth's crust; and the more strictly this inquiry is carried out, the more convinced will the observer become of the great fact, that there is a history to be learnt—a succession of events to be described."

But it may be urged, that the mere fact of order in the arrangement of these superficial materials proves simply, that they were deposited in succession; they might either have been so placed at the creation, or have arranged themselves as we find them during some great deluge. This is precisely the theory of those who object to the doctrine of a preadamite world; but the appearances presented by every quarry and sea-cliff contradict it. All the facts prove, that both the nature of the deposits and the circumstances of deposition were different from either a creation of the surface as it now exists, or the formation of such a surface by a deluge. We find, for example, not only a vast number of strata, of great thickness and extent, but exhibiting endless variety both in the nature and condition of the materials of which each is made up. We find that, in many instances, these beds do not lie regularly upon each other, and from their direction and inclination, are obliged to conclude that one series had time to harden, and be disturbed in position, before another was placed upon it. And, what is a still

more convincing fact, we find further, that almost throughout the whole great and varied series of strata, there are buried the exuviæ of animals, of different kinds, each group presenting in itself a history, and denoting a condition of things fitted to its existence. The proper consideration of these facts, we think with Mr. Ansted, "cannot fail to satisfy any one that the strata of the earth's crust were formed gradually and slowly, under various circumstances, and at different times."

Mr. Ansted gives the following tabular view of the successive geological periods, which will aid the reader in comprehending the details about to be given. The epochs are set down in the order in which they are presented in a descent from the surface towards the interior of the earth. The more recent formations, where they have not been disturbed, must constitute the uppermost part of the crust; as we dig down we reach older strata, containing the remains of more ancient groups of animals, until at length we come to rocks in which no vestiges of life remain. This is the first, which embraces the *primitive* rocks, and stands at the bottom of the table.

*Tabular View of the Successive Geological Periods.*

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| III. The Modern Epoch. | 10. The Period of the caverns and gravel; with Carnivora, the Megaceros and other gigantic ruminating animals, and the elephants of Europe; and of various gigantic animals in Asia, America, Australia, and New Zealand.—(*Newer Tertiary.*) |
| | 9. The Periods of various large animals of the Middle Rhine valley, succeeded by that of the mastodon and elephants in North America, England, Northern Europe, and India.—(*Middle Tertiary.*) |
| | 8. The Period of the pachyderms of the Paris basin, and of the sub-tropical (?) fruits and animals of the London and Hampshire basins.—(*Older Tertiary.*) |

II. The Middle Epoch.
- 7. The Periods of the Chalk and Greensand; during the deposit of which there was probably a deep sea, covering a large proportion of the existing land.
- 6. The Periods of the gigantic land reptiles, the flying reptiles, the gigantic crocodilians, and the first introduction of mammalian animals. (*Wealden* and *Oolite*.)
- 5. The Periods of the frog-like, bird-like, and marine reptiles. (*Lias* and *Trias*.)

I. The Ancient Epoch.
- 4. The Periods marked by the presence of vegetables and the first introduction of reptilian animals.—(*Permian* and *Carboniferous*.)
- 3. The Period of fishes. (*Devonian*.)
- 2. The Period of invertebrated animals.—(*Silurian*.)
- 1. The Period antecedent to the introduction of life.

Whatever may have been the original, chaotic condition of the earth, whether fluid and incandescent from extreme heat, or not, it is fully established that at a certain depth its crust is made up of rocks, crystalline in their character, which were once in a state of igneous fusion. During the whole of the time when these rocks were being formed, it is likely that there were no living beings on the earth. *Granite* and its allied rocks are *azoic*. If any animals did thus early exist and left their remains in them, all such traces of organization have been obliterated by fire. Upon the crystalline rocks repose a class almost as widely extended, themselves often crystalline, but bearing evident marks of having been deposited from water. These may be described as gneiss, mica-schist, and clay-slate, which, like the rocks beneath, are also unmarked by life. When they were deposited we may suppose that our globe began to exhibit alternations of land and water:

"But all was then bare and desolate; not a moss nor a lichen covered the naked skeleton of the globe; not a sea-

weed floated in the broad ocean; not a trace existed even of the least highly organized animal or vegetable; everything was still, and with the stillness of absolute death. The earth was indeed prepared, and the fiat of creation had gone forth; but there was as yet no inhabitant, and no being endowed with life had been introduced to perform its part in the great mystery of Creation."

Wrapping round these igneous rocks, there are found strata, evidently deposits from water, in which life first appears. These sedimentary rocks are expanded sometimes to a thickness of many thousand feet, forming a natural group, the *Silurian*, which may be subdivided into two parts, the lower being the thicker, but the upper containing a greater number and variety of remains of animals.

In looking over the varied organic remains in these older rocks, the first thing that strikes the geological naturalist, is the apparent want of fishes, and in fact, of all the vertebrated animals. The proof is satisfactory enough, that these great thicknesses of mud, sand, and limestone, were formed at the bottom of water, but we search in vain among their fossils for the smallest fragment that could be referred to a fish. Either fishes had not been created, or "the conditions for their development were so unfavorable, that they were extremely rare, and formed no important group among the inhabitants of the sea, in places where other organic remains, often found in newer formations accompanying fishes, are very abundant." The animals preserved in the lower silurian strata are sea-weeds, called *Graptolites*, the habitation, it is supposed, of compound creatures which scarcely deserve the name of animals. Associated with them are the polyps, somewhat more highly organized, "which build those singular and lasting monuments, the coral-reefs;" and also the beautiful *Crinoids*, which are still a step higher in organization. These strata also include a singular and extensive group of crustacean animals, known by the name of *Trilobites*, so called from their shape, as well as bivalve, univalve, and many-chambered shells. The animals that inhabited the

latter shells were carnivorous in their habits, like the cuttle-fish—"a creature of high and complicated organization among the invertebrata, and which seems to have been introduced among the very earliest of the species intended to people the primæval seas."

"All these animals must have been to a certain extent contemporaneous; and it is worth while to remark, concerning them, that they exhibit some instances of very imperfect, and some of the most perfect, development of the great kingdom of nature to which they belong. In the older beds, at least until the termination of the first great epoch, the silurian, there seem, indeed, only to have been introduced successive modifications and additional species of the invertebrated type; and not till its close did the fishes appear, as if preparing the way for the next period, marked by the prevalence of these more highly organized beings."

Insignificant in respect to size, the polyps acquire singular interest from the fact, that during every successive period, from their first appearance in the infancy of the world to the present day, they have been perpetually adding to the solid matter of our globe. They are endowed with the power of separating from sea-water any carbonate of lime contained in it, and this they do although the quantity present be so small as scarcely to be appreciable by delicate chemical tests.

"The prodigious extent of the combined and unintermitting labors of these little world-architects must be witnessed, in order to be adequately conceived or realized. They have built up four hundred miles of barrier reef on the shores of New Caledonia; and on the northeast coast of Australia their labors extend for one thousand miles in length; and these reefs may average, perhaps, a quarter of a mile in breadth, and one hundred and fifty feet in depth, and they have been built amidst the waves of the ocean, and in defiance of its fiercest storms. The Geologist, in contemplating these stupendous operations, learns to appreciate the circumstances by which were deposited in ancient times, and under other conditions than those which now characterize

our climate, those mountain masses of limestone, for the most part entirely coralline, which abound in many parts of our native island. The most abundant remains of corals in these masses are similar in their general nature to living species, but indicate animals very distinct from those living polyps which are now actively engaged in forming similar deposits on the undulating and half-submerged crust of the earth washed by the Indian and Pacific oceans. The limestones, which form a part even of the oldest formations, offer distinct proof, by their organic remains, that they are due to the secretions of gelatinous polyps, the species of which perished before those that formed the newer strata were created; and, as these polyps of the older period have been superseded by those of the present day, so these, in all probability, are destined to give way in their turn to new forms of essentially analogous animals, to which, in time to come, the same great office will be assigned,—to clothe with fertile limestone future rising continents."

The crinoidal animals were possessed of singular beauty, their name implying a resemblance to the lily. Many of them grew upon stems, and with a body in the form of a cup, having extended arms attached, present the appearance of a cup-shaped flower. Although so nearly like the coral polyps in some point of structure, and in the simplicity of their organization, the encrinites belong to a much higher group, and exhibit important resemblances to the star-fishes.

Of the class of Mollusca, all the varieties—univalve, bivalve, and many-chambered—seem to have been introduced with the first tenants of our globe. In the lowest of the silurian strata their shells are found occasionally in vast numbers. These animals, though most simple in their organization, are distinguished from those just described, by the possession of a much more distinct nervous centre. Some of them were carnivorous, and with a complicated shell, composed of a number of separate compartments serving as a float, were enabled to rise at will to the surface of the water, or sink down into the depths of the ocean in search of their food.

The coral polyps, the crinoids, and the shell-fish have each their representatives in the existing seas; the trilobites have disappeared and left no animals of analogous type behind them. They were numerous in the primæval seas, but died out in the coal era. They were a singular race of animals, belonging to the family of which the lobster is a member. Their most remarkable feature was their large compound eyes, constructed on the same principle as those of the drag-on-fly and other insects. They were provided with a large crescent-shaped shield, completely defending the head; their body was secured from the attack of an enemy by a number of plates moving readily upon one another, and the tail was armed with a similar series. The animal seems not to have had antennæ, or legs, but by the aid of its numerous eyes, it was able to see any approaching danger; and the power of rolling itself into a ball, like the wood-louse, "enabled this creature, no doubt, to escape the attack of many of its enemies." The different species, probably, "lived for the most part in shallow water, not buried in mud, but floating near the surface with their under side uppermost, feeding on the minute and perhaps microscopic animalcules that usually abound in such localities." From the complete and complicated contrivance by which these animals, without moving from the spot in which they might be, could see in all directions around them, it is clear that at the earliest period of the introduction of animals, the general conditions of light and the atmosphere could not have differed materially from those which now obtain.

These tribes began at once and together to perform their work on earth. There is no appearance of any regular order of progression, by which the encrinite succeeded the coral polyp, the trilobite the encrinite, and the shell-fish the trilobite. We find their exuviæ all huddled together in the rocks, showing that they were truly contemporaneous. They were ushered thus early upon the stage, there can be no doubt, as the group best adapted to the conditions under which the world existed at the time. Towards the close

of the period we are considering — *the period of invertebrate animals* — a few small fishes were introduced as typical forms of what should afterwards abound. Up to the time of their introduction the shell-fish or trilobites were the lords and tyrants of creation. And strange must have been the appearance of that ancient world. With something of resemblance to our present seas "in the reefs and islands of coral rising gradually to the water's edge, as the coral polyp toiled and labored from day to day and from year to year," as remarked by Mr. Ansted, "there would yet be much more of difference both in the shallows and depths of the ocean. The former sometimes with a sandy, but more frequently a muddy bottom, would be peopled with countless myriads of those unsightly animals, the trilobites, swimming near the surface of the water with their backs downwards, looking out constantly, and sinking at the slightest approach of danger from beneath; while the remains of successive generations of these creatures, mixed with mud and sand, would rapidly form beds sometimes of great extent. From among such beds, or attached to solid rock, would be seen, rising or leaning over on their short and slender stems, the simple forms of the crinoids or stone-flowers," some spreading out their arms and fingers in search of food, others with their cup-like envelope enclosed, presenting every variety of form and clothing the sea bottom with strange beauty. The molluscous tribes, with single and double valves, attached to the rocks, or floating through the water, gave additional variety to the scene.

In the last strata of the silurian system there are found a few remains of fishes, small in size, but of a class presenting the highest organization, so that here, too, the law of progressive development fails. Few in the silurian rocks, they become abundant in the strata immediately superjacent, and are nowhere more numerous than in the *Devonian* system. With their introduction we mark a decline in the trilobites, and certain species of the cephalopodous animals. These crustaceans, and their contemporary orthoceratites, attained

their most gigantic size in the silurian era, and are now growing dwarfish, and soon will disappear. And thus we see, that by a great law of the Creator, species, like individuals, have a certain limited term of existence; they are introduced, last only for a definite period, and are then succeeded by others performing the same office.

We are still in the ancient epoch. There is no evidence that dry land had yet appeared; but upon the devonian system we find a series of formations containing the remains of vegetables. In our ascent from the primitive rocks we have reached the *Carboniferous* system. Coal is so palpably a product of vegetables, that no geologist now doubts its origin. Numerous plants which contributed to its formation have been authenticated, and their roots, leaves, and stems have been so distinctly imprinted upon the slates of the coal-fields, that the flora of the carboniferous period has been satisfactorily determined. It was in this period that the trilobites became extinct. No traces of that group of animals have been detected in the rocks of a later era. Not only so, but as remarked by Ansted,

"Between the close of the older epoch and the commencement of this, which we call the middle, every species, both of animal and vegetable, seems to have been, almost without exception, changed. All the older forms have disappeared; all the modifications up to that time introduced have vanished; many even of the larger groups are so greatly altered, and have become so rare, that they also have nearly died out, either from the lapse of time or change of condition; and we have thus a new creation,—a new world, as it seems, supplying the gap produced by the mighty change, whatever it might have been, which closed one epoch of the earth's history and commenced a second.

Some races, however, lived through the change. The reptiles were introduced in the former epoch, and survived catastrophes by which whole families of fishes and invertebrated animals were swept away, thus furnishing a link

which connects the chain of beings of the two eras. In the middle epoch they reached their full development, and gigantic was the stature to which some of them attained. Along the shores of those ancient seas we find the foot-prints of lizards, turtles, tortoises, and birds. Mr. Ansted draws an imaginary picture of one of these huge reptiles, the labyrinthodon. He says—

"We may imagine one of these animals, as large as a rhinoceros, pacing leisurely over the sands leaving deep imprints of its heavy elephantine hind foot, strangely contrasting with the diminutive step of its shorter fore extremities. Another, a smaller variety, provided like the kangaroo, not only with powerful hind legs but also with a strong tail, also leaves its impress stamped upon the sand, although itself, perhaps, soon fell a victim to the voracity of its larger congener. These and others of their kind, passing over the sands and marking there the form of their expanded feet, marched onwards in their course, fulfilled their part in nature, and then disappeared forever from the earth, leaving, it would seem, in some cases, no fragment of bone, and no other indication of their shape or size than this obscure intimation of their existence."

Birds would appear to have been thus early introduced, for we find their foot-marks mingled in the new red sandstone with those of the lizard and the tortoise, and towards the middle of the epoch the remains of insects also appear. And this, too, was the period at which the mammalia took their place among the inhabitants of our globe. The remains of marsupial animals are found in the deposits of the middle epoch; but neither they nor birds appear to have been numerous, and the great characteristic of the period unquestionably was the existence of reptiles in such vast variety of form, and in such relative numerical abundance.

Quadrupeds of that peculiar class to which the opossum and the kangaroo belong, appear to have been the first mammals introduced, their remains having been found in older deposits than those of any other quadruped. Those which suc-

ceeded them were of the thick-skinned family, now represented by the elephant, the hog, etc., and of the carnivorous species, such as a wolf, a fox, and a raccoon. Mingled with the remains of these animals, the fragments of teeth have been found indicating the existence of contemporary monkeys. Few remains of ruminant animals are discovered in the deposits which contain the bones of the pachyderms, that useful group of quadrupeds having been reserved, it would seem, to make their appearance upon the earth with man.

"The pachydermatous animals that chiefly attract attention amongst the older tertiary quadrupeds are those which have been named *Palæotherium* and *Anoplotherium*. Besides these there are also many others more or less nearly allied. Most of them exhibit analogies with the horse, and connect the heavy pachyderms, such as the elephant and tapir, with the ruminants. They form a well marked group, of which the tapir is perhaps the best living representative."—*Ansted's Ancient World.*

The earth where London and Paris stand witnessed strange revolutions in its humbler inhabitants, before it became the scene of the great drama, some of the acts of which are now passing before our own eyes. The large rivers of those regions were peopled with crocodiles; turtles and tortoises of huge stature floated upon them; wolves, opossums, squirrels, and raccoons, unlike their present congeners, sported in their forests; but among these there was no goat or ox, sheep or camel to be seen. The place of this valuable family was filled by representatives of the tribe of which the hog, the horse, the rhinoceros, and the elephant, are the present types.

"Troops of monkeys might be seen skipping lightly from branch to branch in the various trees, or heard mowing and chattering and howling in the deep recesses of the forest. Of the birds, some clothed in plumage of almost tropical brilliancy, were busy in the forests, while others, such as the vulture, hovered over the spots where death had been busy.

Gigantic serpents might have been seen insidiously watching their prey. Other serpents in gaudy dress were darting upon the smaller quadrupeds and birds, and insects glittered brightly in the sun. All these indications of life and activity existed, and that, too, not far distant from the spots on which are placed the two most important cities in the world. But this happened not only before our island was visited by its earliest human discoverer, but long before man had been introduced on the earth."—*Ansted.*

It is a fact which strikes the mind with force that the animals in existence at this epoch were limited in their distribution; none of them extended over a large surface of the globe, and no one species was decidedly superior to the rest. But we are approaching the *Saturday evening* of creation, when a new order of beings was to appear. Revelation instructs us that man was created at the end of the week, and the facts of geology are in accordance with the teaching of the Sacred record. Comparatively, he and his associated animals are but creatures of yesterday. We strip off from the earth's surface its vegetable mould, and remove with it all human remains, and all that man has built or dug, erected or excavated. His bones and his works go not back to the latest geological period—the *diluvial*, in which the mastodon became extinct. With his creation a new era commenced, and the discoveries in geology bear out the chronology of the Bible, which assigns it a duration of less than six thousand years. A race was now introduced preëminent above all other races of beings—a race fitted to occupy the whole surface of the globe, and which no other race is destined ever to supplant or supersede.*

Let us now turn back for a moment, and review the series in the order in which they are presented to us as we go downwards from the surface. We have stripped away the vegetable mould, with all its living inhabitants, animal and vegetable, and all the exuviæ which they have deposited

* Miller, in his *Old Red Sandstone.*

during the brief term of their existence. Far as the eye can reach, the vast panorama presents but a dreary wilderness of diluvial clays and gravels, with here a bare rock sticking through, and there a scattered group of boulders.* They lie amid sea-shells of the existing species, and the bones of the mastodon are buried among them. The Siberian elephant, found preserved in the ice, belonged to their period, and they rest upon the bones of huge ruminants, and carnivorous animals now extinct.

We remove another layer, and underneath it find reposing the remains of the *xiphodon* and *palæotherium*, the rhinoceros and hippopotamus, and their allied thick-skinned species. Mingled with these are the teeth of monkeys, and extinct species of wolves and foxes, and the bones of birds akin to our vultures, owls, quails, and woodcocks.

The next series brings us to the period of the gigantic reptiles. Birds have left their foot-prints upon the rocks of this era, and the bones of a single marsupial quadruped have been found, but the predominant type in that age of the world was reptilian. Monsters of immense size and strange shapes peopled the ancient seas—lizards provided with wings, and capable of sharing with birds the dominion of the air. This group is traced downwards to its beginning, and the removal of another layer brings us to a formation in which a fish was the master-existence. For long ages all vertebrate existence was comprised in this class of animals, which overspread and took full possession of the seas. But we hasten on, and find the remains of this family becoming rare. We come at length to the ancient silurian system, in which the exuviæ of vertebrate animals first appear—"few and far between, and restricted, so far as is yet known, exclusively to its upper division."† The remainder of the description we give in the beautiful language of Mr. Miller:

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* This is not true of all countries. It is true of England and the northern parts of the United States—of all north of latitude 40°. At about that point the drift appears to have stopped.

† Miller's *England and its People.*

"We pass hurriedly downwards. The vertebrata vanish from creation. We have traced the dynasty to its rude beginnings; and now an ignobler though more ancient race of kings occupy the throne. We have reached, in our explorations, the dynasty of the crustacea. In all creation, as it exists in this period of dusk antiquity, we see nothing that overtops the trilobite, with his jointed mail of such exquisite workmanship, and his prominent eye of many facets, that so capriciously refuses to admit the light through more or less than just its four hundred and ten spherical lenses. The cephalapoda, indeed, may have held with him a divided empire; but the brachiopoda, the pteropoda, the gasteropoda, and the acephala, must have been unresisting subjects, and all must have been implicit deference among the crinoidea, the pennularia, the corals, and the sponges. As we sink lower and lower, the mine of original existence waxes unproductive and poor: a few shells now and then appear, a few graptolites, a few sponges. Anon we reach the outer limits of life: a void and formless desert stretches beyond, and dark night comes down upon the landscape."

No empires recorded in history ever succeeded each other in an order more definite than these dynasties of crustacean, fish, reptile, and mammiferous animals. The trilobite dynasty came into being at the beginning of the palæozoic era, and was long the monarch of the ocean. The fish appeared towards the close of the silurian era, and was the master-existence, not only during the devonian series, but also during the creation represented by the carboniferous period, "with its rank vegetation and green reflected light." During the next succeeding creation, the master-being was a reptile; and during the tertiary formations, the master-existence was a mammiferous quadruped. "Nor," remarks Mr. Miller, "are the periods when single families arose and sank less carefully noted:

"The trilobite family came into existence in the first beginning of the Palæozoic division, and ceased at its close.

The belemnite family began and became extinct with the secondary formations. The ammonite and gryphite, in all their many species, did not outlive the deposition of the chalk. There is one definite period — the close of the Palæozoic era—at which the Brachiopoda, singularly numerous throughout many previous formations, and consisting of many great families, suddenly, with the exception of a single genus, drop off and disappear. There is another definite period—the close of the Secondary era—at which the Cephalopoda, with nearly as few exceptions, disappear as suddenly. At this latter period, too, the Enaliosaurians, so long the monster tyrants of the ocean, cease forever, and the Cetacea take their places: the be-paddled reptiles go off the stage, and the be-paddled mammalia come on."

The evidence for all this may not be clear to the mind of the reader, and he may be disposed to inquire how this theory of successive creations, implying such antiquity of the earth, squares with the narrative given by Moses in the Bible. The question is a grave one, and demands a serious answer. Mr. Miller has considered it at great length in the work before us, and his reply we should think, must be satisfactory to every candid and enlightened inquirer. "No scientific question," he truly remarks, "was ever yet settled dogmatically, nor ever will be." If the question be one in an experimental science, it must be settled experimentally; if in one of numbers, it must be settled arithmetically; if in a science of observation, like geology, it must be settled by the accumulation of facts. It is worse than a vain effort to attempt to settle a geological question other than geologically, as the Church of Rome, in the days of Galileo, found it to settle an astronomical question other than astronomically. How unfortunate for the cause of truth, if the Inquisition had succeeded, as it labored strenuously, to commit the Bible to the belief that the earth occupies a central position in the system, and that the sun performs a daily revolution around it! This the church of that age held to be the declaration of Moses, and suppose it had been able to pledge the verity of scripture to this false astronomy? Of course, the astronomi-

cal facts of the case would have remained unchanged. Geometry would have lent its demonstrative aid to the truth, that the central position is occupied by the sun, and that the lesser body moves round the larger; the "higher minds would have become convinced for themselves, and the great bulk of the lower, at second hand, that the scripture pledge had been given, not to scientific truth, but to scientific error." It is easy to see how Revelation would have suffered by being placed in a position so essentially false.

The bearing of this history upon the question before us is sufficiently palpable. It was assumed by the older church that Moses taught the only true astronomy. It is contended by some now in the church, that he has given us the only true cosmogony. We suppose that the object of Moses was not in either case to teach astronomy or geology, but to impart that great moral and religious system, heaven-inspired, of which man everywhere has entertained the expectation. His mission was to teach man "what God is, and what God demands of him." He spoke of the sun's rising and setting, as it appeared to other men to do; he did not speak astronomically, but naturally. And so in regard to the earth, his purpose was not to inform us of its chemical composition, or of the succession and relative age of its strata, but simply that "in the beginning, *God* created the Heavens and the Earth." The Egyptians held that they were self-existent, self-created. Moses meant to subvert this doctrine, and to teach his people that they were the creation of that Being whom they worshipped as God. He did not tell them when, but by whom the world was created. Chronologists date back the creation of man about six thousand years, and there is nothing in geology that traverses that belief. All that has hitherto been discovered bears testimony to the fact, that he came late upon the earth, and has been contemporary with but one creation. Evidently, it was this creation that the Jewish historian had in his mind, as it was of the animals of this era that he spake as having perished in the flood. These races, preserved through man's agency, live to bear him company

in his pilgrimage, and minister to his comfort and necessities. But the tribes of the former eras, the trilobites, crinoids, and strange fishes, buried whole miles beneath the surface, is it necessary to suppose that he meant to include these in the same creation? "In the beginning," all alike, those of the human era, and those "embedded in rock and transformed into stone," came from a Divine hand; but all were not created just six thousand years ago. Moses was utterly silent on the contemporary creations of Mars and the other planets; why then should he be expected to have treated of the creations of the silurian, devonian, and carboniferous periods? Seeing that the truths of astronomy were withheld, what right have we to look in the Sacred record for those of geology?

As the world has at length consented to allow astronomers the right to decide in all astronomical questions, so geology must, in the end, be permitted to settle all geological ones; and in conceding this right to science we detract nothing from the sacredness of Revelation. We are fully persuaded of the truth of the remark made by Mr. Miller on this point, that "the infidel has nothing to hope, the christian nothing to fear, from the great truths of geology."

It is an easy matter for men to profess any form of doctrine, but true belief is the result of evidence. Men cannot believe as they please. If they were compelled to swear, as in Galileo's case, that the earth is the central body, would they believe it? With the thousand strata of rocks, miles in depth, with their contents of organic remains before the eye, is it reasonable to ask men to believe that the world might have been called into existence yesterday? Let us look at the argument; it is forcibly presented by Mr. Miller in the following extract:

"We stand in the middle of an ancient burying-ground in a northern district. The monuments of the dead, lichened and gray, rise thick around us; and there are fragments of mouldering bones lying scattered amid the loose dust that

rests under them, in dark recesses impervious to the rain and sunshine. We dig into the soil below: here is a human skull, and there numerous other well-known bones of the human skeleton,—vertebræ, ribs, arm and leg bones, and those of the jaws, breast, and pelvis. Still, as we dig, the bony mass accumulates;—we disinter portions, not of one, but of many skeletons, some comparatively fresh, some in a state of great decay; and with the bones there mingle fragments of coffins, with the wasted tinsel-mounting in some instances still attached, and the rusted nails sticking in the joints. We continue to dig, and, at a depth to which the sexton almost never penetrates, find a stratum of pure sea-sand, and then a stratum of the sea-shells common on the neighboring coast, —in especial, oyster, muscle, and cockle shells. It may be mentioned, in passing, that the churchyard to which I refer, though at some little distance from the sea, is situated on one of the raised beaches of the north of Scotland; and hence the shells. We dig a little further, and reach a thick bed of sandstone, which we penetrate, and beneath which we find a bed of impure lime, richly charged with the remains of fish of strangely antique forms. 'The earth, for anything that appears to the contrary, might have been made yesterday!' Do appearances such as these warrant the inference? Do these human skeletons, in all their various stages of decay, appear as if *they* had been made yesterday? Was that bit of coffin, with the soiled tinsel on the one side, and the corroded nail sticking out of the other, made yesterday? Was yonder skull, instead of having ever formed part of a human head, created yesterday, exactly the repulsive-looking sort of thing we see it? Indisputably not. Such is the nature of the human mind,—such the laws that regulate and control human belief,—that in the very existence of that churchyard, we do and must recognise positive proof that the world was *not* made yesterday.

"But can we stop in our process of inference at the mouldering remains of the churchyard? Can we hold that the skull was not created a mere skull, and yet hold that the oyster, muscle, and cockle shells beneath are not the remains of molluscous animals, but things originally created in exactly their present state, as empty shells? The supposition is altogether absurd. Such is the constitution of our minds, that we must as certainly hold yonder oyster-shell to have once formed part of a mollusc, as we hold yonder skull to have once formed part of a man. And if we cannot stop

at the skeleton, how stop at the shell? Why not pass on to the fish? The evidence of design is quite as irresistible in them as in the human or molluscous remains above. We can still see the scales which covered them occupying their proper places, with all their nicely designed bars, hooks, and nails of attachment: the fins which propelled them through the water, with the multitudinous pseudo-joints, formed to impart to the rays the proper elasticity, lie widely spread on the stone; the sharp-pointed teeth, constructed like those of fish generally, rather for the purpose of holding fast slippery substances than of mastication, still bristle in their jaws; nay, the very plates, spines, and scales of the fish on which they fed, still lie undigested in their abdomens. We cannot stop short at the shells: if the human skull was not created a mere skull, nor the shell a mere dead shell, then the fossil fish could not have been created a mere fossil. There is no broken link in the chain at which to take our stand; and yet having once recognised the fishes as such,—having recognised them as the remains of animals, and not as stones that exist in their original state,—we stand committed to all the organisms of the geological scale."

Here we bring our article to a close, not without misgivings that we have already extended it too far for the patience of some of our readers. If any of them feel an interest in the subject we refer them to the works of Mr. Ansted and Mr. Miller. The former is easily accessible, and will be found an admirable introduction to the study of geology. The latter, we trust, will be republished by some of our booksellers. It is the production of one of the first minds of the age.

ART. V.—*On Disorders of the Cerebral Circulation; and on the Connection between Affections of the Brain and Diseases of the Heart.* By GEORGE BURROWS, M.D., Late Fellow of Caius College, Cambridge; Fellow of the Royal College of Physicians, London; Physician and Lecturer on the Principles and Practice of Medicine, at St. Bartholomew's Hospital. WITH COLORED PLATES. Philadelphia: Lea & Blanchard. pp. 216. 8vo. 1848.

The materials of which this volume is composed formed the substance of the Lumleian Lectures before the Royal College of Physicians in 1843 and 1844. The doctrines broached in these lectures having attracted favorable notice from many whose opinions were highly valued, the author was led to revise and submit them to the consideration of the profession.

In the first section, "on the peculiarities of the circulation within the cranium," Dr. Burrows effectually demolishes the theory, embraced by many distinguished physicians, that the absolute quantity of blood within the cranium is at all times nearly the same. This doctrine appears to have been first asserted by Monro, of Edinburgh; he explained it by reference to the peculiar structure of the cranium, and used to illustrate it by exhibiting a glass globe filled with water, and desiring his pupils to remark that not a drop of fluid escaped when the aperture was inverted. Abercrombie, in his work on Diseases of the Brain and Spinal Cord, inculcates similar views; and many writers and lecturers have since adopted them. Abercrombie's opinions are primarily founded on certain appearances observed by Dr. Kellie in the brains of animals bled to death, and are sustained by reference to the peculiarities in the structure of the head. Clutterbuck in the Cyclopedia of Practical Medicine carries the theory still further, and maintains that no abstraction of blood can lessen the quantity of blood in the cerebral vessels. It is worthy of remark that these views are confined almost wholly to the British writers; they have also found some advocates in this

country; but they have been received with little favor either by the French or Germans.

The author gives an abstract of some of the experiments performed by Dr. Kellie, and shows very plainly that they do not warrant the inferences drawn from them. These inferences appear to have been adopted by Abercrombie and others, without any examination of the evidence supporting them. Dr. Kellie's first proposition is, "that a state of bloodlessness is not discovered in the brains of animals which have died by hemorrhage; but on the contrary, very commonly a state of venous cerebral congestion:" and a few of the experiments on which it is founded are thus given:

"*Exp. E.*—In this experiment both carotids of a sheep were tied, and four minutes after the jugular veins opened. The quantity of blood lost was thirty-eight ounces, when the animal died. The heart contained no appreciable quantity of blood. The sinuses of the brain were in their usual state; those at the basis contained less blood than had been found in similar experiments, and the veins on the hemispheres were less filled; the choroid plexus was pale and empty: the vessels on the basis of the cerebrum were better filled, and those on the basis cerebelli were minutely injected."

"*Exp. H.*—'A dog was bled to death from the carotids, having lost thirty-seven ounces of blood. The viscera in general were all drained of their blood. The dura mater contained little blood: the lateral sinuses were, however, well filled. On the pia mater were several vessels of a florid color, but not turgid. This brain seemed upon the whole more depleted than usual.'

"Now it will be interesting to contrast the appearances of the vascular system of the brain in the two foregoing experiments, where the animals died by hemorrhage, with the condition of the brain in two other animals, where death was caused by other means.

"*Exp. L.*—Both carotids and both jugulars were tied in a dog, an operation which it survived twelve hours. The vessels of the dura mater were remarkably turgid, and all the sinuses much loaded with blood. Both the larger and the smaller vessels of the pia mater were fully injected with red

blood. Not only the pia mater through its whole extent, but the cineritious substance, had a suffused, reddened, and, as it were, blood-shot appearance. 'In short,' writes Dr. Kellie, 'this brain was gorged with blood in all its minuter vessels,' and was obviously in a very different state of vascularity to to those of the animals bled to death in the experiments E and H.

"Let us analyse another experiment (M). In this a dog was poisoned with prussic acid. 'The sinuses and veins were found loaded and congested, and the brain was every where turgid with blood. It was quite evident,' writes Dr. Kellie, 'that this brain, and that of the dog (L), contained, beyond all doubt or dispute, a much larger quantity of red blood than the brains of any of the animals which had been bled to death. These comparative experiments afforded us the most satisfactory proof that the other brains had been really depleted by bleeding, and their vessels drained of a very sensible proportion of the red blood usually contained by them.' "

It seems that, notwithstanding the statements here made by Dr. Kellie, he affirms, in a subsequent paper, published in the Medico-Chirurgical Transactions, "that in the ordinary state of the parts we cannot lessen, to any considerable extent, the quantity of blood within the cranium; whereas, if the skull of an animal be trephined then hemorrhage will leave very little blood in the brain." This discrepancy between the results of experiments and subsequent statements, led Dr. Burrows to repeat some of Kellie's experiments.

"On the 11th of January, 1843, I killed two well-grown rabbits. The one by opening the jugular vein and carotid artery on one side of the throat; the other was strangled. Each animal died violently convulsed. A ligature was drawn tightly round the throat of the rabbit (A) immediately it expired, to prevent any further escape of blood from the vessels of the head. The rabbits were allowed to remain twenty-four hours on a table, resting on their sides.

"While the blood was flowing from the rabbit (A), the conjunctiva was observed to become pallid, and the eyeballs to shrink within the sockets. Upon the examination of the

head of this rabbit, the integuments and muscles appeared blanched and exsanguined. Upon removing the upper portions of the cranium, the membranes of the brain were found pallid, and scarcely the trace of a bloodvessel was to be detected on the surface of the brain. The longitudinal and lateral sinuses were nearly empty of blood, and, their course was not denoted by any color of blood. Upon making sections of the brain, the interior appeared equally exsanguined.

"Soon after the cord was drawn tight round the throat of the rabbit (B), the conjunctival vessels became congested, the eyeballs turgid, prominent, and even projecting beyond the margin of their sockets. The integuments and muscles of the head were found full of blood. Upon opening the cranium, the superficial vessels of the membranes, as well as the sinuses, were full of dark liquid blood. The whole substance of this brain, and its membranes, appeared of a dark reddish hue, as if stained by extravasated blood. The contrast between the two brains in point of vascularity, both on the surface and anterior, was most striking. In the one scarcely the trace of a bloodvessel was to be seen; in the other every vessel was turgid with blood.

"It seems hardly necessary to bring forward further evidence to prove that death by hemorrhage has a most decided effect in depleting the vessels, and reducing the quantity of blood within as well as upon the outside of the cranium. However, I have repeated the experiments with similar results. In fairness to Dr. Kellie I should state, that I have attended at the slaughtering of sheep by butchers, and find the brains of these animals much less depleted than the brains of rabbits which have died by hemorrhage. But these sheep did not die from simple loss of blood; but partly from division of the pneumogastric nerves and cervical portion of the spinal cord. These lesions, no doubt, influenced the appearances.

Thus Dr. Kellie's first proposition is supported neither by an examination of his original experiments, nor by the results obtained from a repetition of them. A reference to the pathological appearances observed in those suffering from diseases of the brain affords quite as little support. In 53 out of 72 insane patients examined by Mr. Lawrence, the vessels of the brain were found congested, sometimes extreme-

ly so; they were found "unusually empty" but once, in a patient that bled to death from the bursting of an aneurism. "Hence it is not a fallacy, as some suppose, to assert that bleeding diminishes the actual quantity of blood in the cerebral vessels."

Dr. Kellie's second proposition is that, in consequence of the mechanical structure of the cranium, the quantity of blood in the cerebral vessels is not affected by gravitation, or posture of the head.

"In order to ascertain, as far as such an experiment can do, the total effect of the gravitation of the blood upon the vessels of the brain, Dr. K., immediately after administering a destructive dose of prussic acid to two dogs, suspended the one by the heels, and the other by the ears. He allowed them to remain thus suspended for eighteen hours, when they were taken down for examination.

"The effects of posture on the parts exterior to the skull Dr. K. reports to be very great. In the former animal the integuments and their vessels were filled and congested to the greatest possible degree; the integuments of the head of the second dog were pale, and the vessels empty. 'Within the head,' continues Dr. K., 'the contrast is but trifling. The sinuses beyond all doubt were loaded in the first case, and rather empty in the other; the difference of appearance in other parts of the brain was but little striking."

Dr. Kellie's own words led to the belief that gravitation had more effect on the quantity of blood in the cerebral vessels than he was willing to admit. Our author therefore repeated his experiment.

"On the 28th of December, 1842, two full-grown rabbits were killed by prussic acid, and, while their hearts were still pulsating, the one (C, plate 3), was suspended by the ears, the other (D, plate 4), by the hind legs. They were left suspended for twenty-four hours; and, before they were taken down for examination, a tight ligature was placed round the throat of each rabbit, to prevent, as effectually as was possible, any further flow of blood to or from the head, after they were removed from their respective positions.

"In the rabbit (C) the whole of the external parts of the head, the ears, eyeballs, etc., were pallid and flaccid; the muscles of the scalp, and bones of the cranium were also remarkably exsanguined. Upon opening the cranium, the membranes and substance of the brain were pallid, the sinuses and other vessels were exsanguined; anemic beyond my expectation.

"In the rabbit (D) the external parts of the head, the ears, eyeballs, etc., were turgid, livid, and congested. The muscles and bones of the cranium were of a dark hue, and gorged with blood, which at some parts appeared extravasated. Upon opening the cranium, the membranes and vessels were dark and turgid with liquid blood; the superficial veins were prominent, the longitudinal and lateral sinuses were gorged with dark blood, and there was staining of the tissues, if not extravasation of blood into the membranes. The substance of the brain was uniformly dark, and congested to a remarkable extent.

"Dr. Kellie asserts, but I think his experiments do not support him, that the contrast in the appearances within the heads of the two animals was but trifling. In my analogous experiments the contrast was most striking. In the one was to be seen a most complete state of anemia of the internal as well as external parts of the cranium; in the other a most intense hyperemia or congestion of the same parts; and these opposite conditions in the vascularity of the brain induced solely by posture, and the consequent gravitation of the blood."

It may be affirmed, then, that the brain is not exempt from the physical law of gravitation of the fluids to the lowest part of the body; and hence some caution is necessary to avoid being deceived by the appearances that may be found in the brain, according as the head is elevated or depressed during the examination. The author suggests also that, in cases where it is desirable to ascertain the precise amount of congestion of the cerebral vessels at the time of death, a ligature should be placed around the neck and drawn sufficiently tight to compress the cervical vessels and arrest the flow of blood through them.

Dr. Kellie appeals in the third place, in support of his views, to the absence of congestion in the brains of those

cessary for these purposes, the large vessels of the neck are divided; and then while the head is elevated to remove the skull-cap, the blood, yet fluid, leaves the cranium and passes through the divided vessels of the neck into the chest.

Dr. Kellie's fourth proposition is that if there be repletion or depletion of one set of vessels (arteries or veins) in the cranium, there will be an opposite condition of the other set of vessels.

"Those who maintain that the absolute quantity of blood in the cerebral vessels does not vary, admit a disturbance of the cerebral circulation in the following manner:—They point out the probability of a frequent alteration in the relative quantities of blood in the cerebral arteries and veins. Thus, they assert that those pathological states which have a tendency to cause influx of blood into the cerebral arteries, and accumulation in those vessels, will accomplish this change at the expense of the cerebral veins. Again, anything causing obstruction of the return of blood from the cranium will produce fulness of the sinuses and cerebral veins, but, at the same time, the quantity in the arteries will be equally diminished. They also maintain, if the quantity transmitted to the brain be lessened, the cerebral arteries will be comparatively empty; but there will be a corresponding fulness of the venous system within the cranium."

But the author refers to his experiments, already detailed, and denies that any such venous congestion exists. Algebra has been appealed to, to show that if there is an increase of blood in one set of vessels, there is a diminution in the other:

"Thus it is said, if the whole quantity of blood within the cranium $=C$, a constant, the quantity of blood in the arteries $=x$, the quantity of blood in the veins $=y$, then $C = x+y$ always; but let $x$ be diminished by a quantity $(a)$, then $y$ must be increased by this quantity $(a)$, otherwise the original supposition, $C = x+y=(x-a)+(y+a)$ would not be maintained."

But again, the results of experiments are conclusive in regard to these speculations. The error in all these consists in the assumption that the quantity of blood within the cerebral vessels is a constant quantity. Both experiments and physiological considerations show that it varies at different times and under different circumstances. The author thinks that those who maintain the doctrine which he is combatting, have not given due importance to the cerebro-spinal fluid, which according to Magendie is never less than two ounces in the healthy adult, and often amounts to five ounces in persons of large frame.

"Regarding this serum as an important element of the contents of the cranium, I admit that the whole contents of the cranium, that is, the brain, the blood, and this serum together, must be at all times nearly a constant quantity.

"But variations in the quantity of serum within the cranium are quite compatible with health; and in morbid states of the brain we know that at one time the cerebral substance, its membranes, and ventricles, will be nearly devoid of serum; while, at another time these parts abound with serous effusion. In accordance with the variations in the quantity of extra-vascular serum, there must be fluctuations in the quantity of intra-vascular fluid, the blood. From this consideration alone, it seems that the blood may be increased or diminished in the cranium. The increase or decrease may effect the two systems of vessels, arterial and venous, equally, or the equilibrium may be disturbed; there may be excess in either arteries or veins, without any necessary diminution in the quantity of the blood contained in the other set of vessels."

Another point, which has been much insisted on from the time of Monro to the present day, is the supposed influence arising from the mechanical conformation of the cranium:—

"Does the anatomical structure of the human cranium warrant the opinion that it is a complete sphere, capable of removing its contents from the influence of atmospheric pressure? I think not. The numerous fissures and foramina for the transmission of vessels and nerves through the bones of

the cranium appear to me to do away with the idea of the cranium being a perfect sphere, like a glass globe, to which it has been compared by some writers. If there were not always an equilibrium of pressure on the parts within and without the cranium, very serious consequences would arise at the various foramina of the skull. Are, then, the contents of the cranium removed from the influence of atmospheric pressure? I think not, from other considerations. Atmospheric pressure is undoubtedly exerted on the blood in the vessels entering the cranium. This pressure, by a well-ascertained law in hydrostatics, must be transmitted in all directions through the fluid blood, and hence to the blood and other contents within the cranium. If, in the natural state of the parts, the brain is defended from atmospheric pressure, should we not expect to find the functions of that organ disturbed in some way, when part of the walls of this sphere is wanting? But in children with open fontanelles, and in adults who have lost part of the bones of the cranium, we observe no peculiar disturbance of the functions of the brain from this gap in the walls of the imaginary sphere. But, lastly, the effects of gravitation on the fluid contents of the cranium, and the effects of the cupping-glasses,* which will often draw blood from the vessels of the dura mater, causing ecchymosis there, assures us that the cranium is not a perfect sphere in the sense in which it has been supposed."

The subject next discussed by the author is that of "vascular pressure within the cranium, and its influence on the functions of the brain."

He thinks it is probable that the functions of the brain, in the healty state, cannot be maintained without a certain amount of pressure on the cerebral substance. These functions appear to be affected by any variation of pressure artificially produced. As the cerebral substance is defended from external causes of pressure, it can only be subjected to

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* "If a patient die soon after blood has been abstracted from the scalp by cupping, and the head be immediately opened, all the exterior and interior anastomosing bloodvessels of the scalp and investing cerebral membranes will be found highly injected to a circumference correspondent with that of each glass," (Commentaries on Insanity, p. 594. G. Man. Burrows, M.D.)

those acting from within. The chief source of this pressure is the momentum of the blood distending the arteries and veins ramifying in the membranes and substance of the brain.

But it is objected, that the substance of the brain is composed of inelastic fluids which are incompressible; and that the brain is incompressible by any force that can be conveyed to it through the carotid and vertebral arteries.

As to the first objection, it confounds *elasticity* and *compressibility*, two properties which are totally distinct, and which bear no constant relation to each other. Elasticity may exist in connection with a very high or very low degree of compressibility. "Thus an ivory billiard-ball is very incompressible, but highly elastic; India rubber is more compressible, and very elastic; and again, a sponge is very compressible, but also highly elastic." Now the contents of the cranium though but slightly compressible are very elastic.

With regard to the second objection, the only force which can act on the brain through the momentum of the blood is partly derived from the contractile power of the left ventricle of the heart, and partly from the reflux of the venous blood during expiration. The existence of a distending force acting within the arteries at every part of the body is generally conceded; the dilatation of the vessels is chiefly owing to this force; the dilatation, according to Parry, being in the direct proportion of the momentum of the blood impelled into the vessels. Now, this dilatation, continues our author, must be a cause of outward pressure on the surrounding tissues in all parts of the body; but while the tissues of other organs swell and expand in proportion to the momentum of the blood in their vessels, the brain cannot expand because it is confined within the limits of its bony case. But if a portion of the cranium be removed artificially, or through accident or disease, it was long since observed that with each systole of the ventricles, the surface of the brain, covered by the dura mater, rises a little, and has a tendency to transgress the limits of its osseous parietes; while during the dias-

tole, the brain shrinks, and the dura mater subsides. That these movements depend on the momentum of the arterial blood may be proved by making pressure upon the arteries going to the brain, or tying a ligature around them. In a case of extensive necrosis of the skull it has been seen that these movements became weaker when pressure was made upon the carotids, and were rendered almost imperceptible by increased pressure. These pulsations of the dura mater were observed by Richerand, Bichat, and Ecker, in dogs which they had trephined; when ligatures were placed around the carotids, the pulsations diminished, and when the vertebral arteries were tied at the same time, the pulsations ceased, and the animal died. Kellie observed, in one of his experiments, that after division of one of the carotid arteries, there was a gradual sinking of the brain from the level of the cranium, as the quantity of blood was lessened and the energy of the heart was impaired. Again; during syncope these movements of the brain cease, to recur with renewal of the heart's action.

"From these observations and experiments, it appears that the brain receives an impulse from the shock of the arterial pulse, which causes a heaving upwards, as well as a short quick expansion of its mass; afterwards, at the moment of the venticular diastole, a collopse of the organ ensues. Also, if the dura mater be exposed to view, and observed during the period of expiration when the free return of venous blood from the brain is impeded, and a larger quantity arterial of blood is distributed with increased force towards that and other organs, the surface of this membrane is seen to rise: the brain itself swells and becomes turgid, but again subsides with the succeeding inspiration. All physiologists who have considered this subject agree in regarding these latter respiratory movements of the brain as partly, if not wholly, attributable to the reflux of the blood in the veins during expiration."

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"But the effect of this reflux of the blood in the veins upon the cerebral substance are still more manifest after those

accidents where, with loss of a portion of the bones of the cranium, there is also a laceration of the dura mater. In such cases the distending forces of the vessels acting on the cerebral substance are so strongly exhibited, that not only is the alternate rising and sinking of the exposed surface observed to correspond with each expiration and inspiration, but portions of the substance of the brain are actually protruded through the opening in the dura mater and bones of the cranium. Hernia of the convolutions of the brain is thus effected by a force from within, just as hernia of the convolutions of the intestines is produced by analogous forces, when the walls of the abdomen are accidentally perforated."

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"If the walls of the cranium were soft and yielding like those of the abdomen, the cranium would expand, and its capacity be increased in proportion to the increased volume of the brain at each expiration, during which period that organ is the seat of an increased afflux of blood: again, when the brain was diminished in volume during inspiration, the walls of the cranium would follow this subsidence, and its capacity would be proportionately lessened. But as the walls of the skull are rigid and inexpansive, when the brain has this recurring tendency to increase in volume, the walls around oppose the expansion, and sustain this centrifugal, or excentric pressure, which is reflected back upon the substance of the organ."

Now, admitting with Abercrombie and others that the brain is incompressible, it must nevertheless feel and sustain the pressure of this distending force as much as if it were compressible. Monroe, as quoted by the author, says truly that "the less compressible we suppose the substance of the brain to be, the more readily we understand how the whole of it may be affected by a plethora, or increased momentum of blood in it."

If we have rendered our author's course of argument thus far intelligible, the reader perhaps will concede that the brain "not only contains a variable amount of blood at different times, but is also subjected to a constant vascular pressure."

This vascular pressure will vary according to the numerous causes that may affect the momentum of the blood in the

cerebral vessels; at one time excessive, suspending the functions of the organ; at another deficient, proving inadequate to sustain these functions. The brain is protected from the injurious effects of these modifications of pressure, partly by the venous system of the cranium and spinal canal, which affords ready exit to redundant blood, and partly by changes in the site of the *cerebro-spinal* fluid.

The presence of a serous fluid within the ventricles and membranes of the brain and the theca of the spinal cord, is a physiological as well as pathological fact. It has already been stated that according to Magendie this fluid varies in amount from two to five ounces. The anatomy of the brain and spinal cord, shows how easily it may descend from the former to the latter, and ascend from the latter to the former. The lateral ventricles communicate with the spinal canal through the third and fourth; and the opening of the latter into the theca of the cord, has been found by Longet large enough to admit the little finger in cases where the fluid was redundant. Communication between the membranes of the brain and spinal canal is still easier. The movements of this fluid are, however, not a matter of conjecture but observation. In children affected with spina bifida, the spinal tumor swells during prolonged expiration, crying, and coughing; if pressure be made upon it with one hand, and the fontanelles examined with the other, in proportion as the spinal swelling decreases the brain swells, accompanied with the symptoms resulting from pressure on the brain and spinal cord. Magendie, Ecker, and others, after having exposed the spinal theca in experiments on animals, observed it alternately rising and sinking, corresponding with expiration and inspiration. The force producing these movements would seem to be considerable, as when the arachnoid of the cord was punctured the fluid spirted out to a considerable distance. Ecker, who investigated this fluid particularly, arrived at the conclusion that it is engaged in a perpetual alteration of ascent and descent.

"At the moment of expiration, the vertebral sinuses, which are numerous and ramifying exterior to the theca, are distended with blood. This distension must cause an approximation of the theca towards the spinal cord, and this inward or centripetal movement will create a pressure upon the fluid within the theca. This contained fluid seeks an outlet, which it finds more readily towards the inside of the cranium than elsewhere. The cerebral veins are, indeed, distended at this same period, but the unyielding sinuses within the cranium are not dilated in proportion: the spinal fluid can thus partly escape in this upward direction; it flows in part into the ventricles, and a part probably beneath the arachnoid on the surface of the encephalon. At inspiration the vertebral sinuses empty themselves, the fluid returns to the vertebral canal, and again occupies the vacated space."

From all these observations and experiments, which we have but glanced at, Dr. Burrows, concludes that this cerebro-spinal fluid is "supplemental to the other contents of the cranium; at one time giving place to an increased quantity of blood; at another making up for a deficiency of blood in the vessels of the head." It is supplemental also of the quantity of nervous matter in the head; being remarkably deficient in hypertrophy of the brain, but existing in large quantity in atrophy of the organ. It may also serve, he thinks, to equalize pressure over the whole cerebro-spinal mass; the ventricles of the brain being contrived, as suggested by Home, for the reception of this regulator of pressure. Magendie had already suggested this as one of its offices in reference to the spinal marrow.

"The foregoing inquiry will elucidate many interesting phenomena observed in states of vascular congestion or depletion within the cranium. When arterial or venous congestion of the brain is suddenly induced, the first effect will be an increased pressure on all the contents of the cranium: this pressure will cause the expulsion of a portion of this extra-vascular serum into the spinal canal. On the other hand, when abstraction of blood from the cranium is effected, there is a tendency to shrinking of the encephalon; but the vacated space is immediately occupied by a certain quantity

of this serum. But when the power of the heart is inordinately increased for any length of time by stimulants, general plethora, or hypertrophy of the left ventricle, a train of symptoms is remarked similar to those produced by varying degrees of mechanical pressure artificially exerted on the brain."

These effects will be most striking where there is pre-existing structural disease within the cranium.

"In those pathological states of the encephalon where there is an increase in the quantity of solid matter within the cranium and a diminution of the quantity of extra-vascular serum, as in hypertrophy of the brain, tumors and cysts in that organ, and in large extravasations of blood on the surface, every cause which is capable of exciting the heart's action produces a notable increased disturbance of the functions of the brain. The variable character of the symptoms of cerebral disturbance in these permanent lesions within the cranium are thus probably accounted for by the variable vascular pressure. Andral has offered a nearly similar explanation of the occasional recurrence and intermittent character of cerebral symptoms, although their supposed cause, organic disease in the cranium is permanent. It seems to me probable that many permanent structural lesions within the cranium do not affect the functions of the brain by pressure, except when there is some cause in operation capable of inducing vascular congestion, or when the lesion is of a mechanical nature, or is gradually increasing."

Such a diminution of the contractile power of the heart as would afford relief to the cerebral symptoms in these morbid states would, in the healthy state, cause failure in the functions of the organ from insufficient vascular pressure—would produce *syncope*. Syncope, according to the author, "is occasioned by insufficient vascular pressure on the brain, and not from the inadequate quantity of blood supplied to the brain as is commonly supposed":

"In the most simple form of syncope, that which arises from a strong moral emotion in a previously healthy person.

we see the single effect of want of due vascular pressure on the brain. In such cases the quantity and quality of the blood in the person's body are unaltered previous to the syncope, and the suspension of the cerebral functions arises solely from the diminished energy of the heart. The blood is no longer propelled with sufficient force from the left ventricle to maintain an adequate pressure on the cerebral substance. If such a person happen to be in the erect posture, the syncope is more rapid and complete; because the enfeebled muscular tissue of the heart is unable to sustain the arterial current of blood against the force of gravity, and preserve the proper degree of pressure on the brain. Let such a fainting person be placed in the horizontal posture, and, as is well known, consciousness quickly returns. But why? Because the enfeebled heart is equal to maintain the arterial current of blood in that favorable posture, and with it the requisite amount of vascular pressure on the brain. The same phenomena occur in syncope produced by hemorrhage, although in this case it is not so easy to show that the suspension of the cerebral functions is occasioned solely by want of vascular pressure on its substance. It has been forcibly pointed out by Dr. Marshall Hall, that bleeding in the erect posture is the best method of ascertaining the power of the system to sustain the loss of blood, and of the propriety of bleeding as a remedial agent. It is a matter of daily experience that a small amount of blood taken from a person in the erect posture will cause syncope; whereas double the amount of blood might have been taken from that person in the horizontal posture without producing a similar result. Hence the posture of the individual, and not the amount of blood abstracted, is the more efficient cause of the syncope. The effects of the erect posture in modifying the influence of the heart on the brain I have just described."

In diseases marked by debility, syncope, sometimes terminating fatally, follows the assumption of the erect posture. During the prevalence of cholera death often occurred immediately on this change of posture, the enfeebled heart being unable to overcome the force of gravity in the carotid and vertebral arteries.

In some persons fainting is produced when the arms are held aloft above the head in a vertical position. The late Dr.

James Gregory, of Edinburgh, could thus produce fainting at will. In these cases the momentum of the blood in the cerebral arteries is diminished, the contractile force of the heart being partly spent in throwing the blood into the arteries of the raised extremities.

Dr. Burrows insists the more on this explanation of syncope because in an authoritative work, (the Library of Medicine), it is asserted that syncope differs from apoplexy only in the extreme feebleness of the heart's action, that both affections are due to the same cause, viz: pressure on the brain, which in syncope is owing to fullness of the cerebral veins, occasioned by diminished action of the heart. "Would the practitioner," asks the author, "attempt to overcome syncope by the use of remedies likely to diminish pressure on the brain? Would he be bold eneugh to place his fainting patient in the erect posture, or draw blood from the jugular veins?"

The remarkable train of nervous symptoms taking place in general anemia, arises not so much from the altered qualities or insufficient quantity of the blood, as from impaired vascular pressure on the substance of the brain. Simple anemia does not produce these symptoms. The most perfect example of simple anemia is that which occurs in hypertrophy of the brain: the organ is completely devoid of blood and serum; the cranium being so filled by the increase of nervous matter, that the blood is unable to make its way through the vessels; yet we witness none of the nervous symptoms of general anemia. There is no lack of vascular pressure here; the heart is unimpaired in power, and its whole force is expended on the cerebral substance, as the blood cannot enter the capillaries.

But in general anemia, such as arises from profuse hemorrhage, there is a fearful train of symptoms; and it is remarked that these are alleviated by whatever has a tendency to increase or assist for a time the power of the heart, and aggravated by whatever has a tendency to lessen or oppose the force with which the blood is propelled into the arteries.

Some curious instances are given of the effect of posture in exciting the functions of the brain. Abercrombie relates the case of a man, extremely weak and emaciated, who in the erect posture, sitting or standing, was very deaf; but when lying down, with the head very low, he heard perfectly. If when standing he stooped forward, so as to produce flushing of the face, his hearing was perfect; and upon raising himself again, he continued to hear distinctly as long as the flushing continued; as this went off, the deafness returned. Here the varying amount of vascular pressure was the cause of the suspension and restoration of the cerebral functions.

Allusion is also made to the fact that many men remarkable for their mental powers, have enjoyed the most rapid flow of brilliant ideas while they have been resting in the horizontal posture. In these cases it is probable that the heart was deficient in power, and the circulation therefore languid.

Combe instances a boy, eleven years old, with feebly-developed chest and heart, who "seemed to be two different characters when sitting up or lying down. In the former attitude, when the head is scantily supplied with blood, he looked apathetic, and sullen; in the latter, the circulation being assisted by the force of gravity, he was animated, talkative, and highly intelligent."

Bricheteau mentions the case of a student, whose memory was treacherous, and who used to place himself with the head depending and the lower extremities elevated when he wished to study with most effect.

In the same way the action of Darwin's horizontal revolving bed is explained. This was invented for the purpose of tranquilizing the nervous system and procuring sleep, and subsequently was employed to calm the violence of maniacs.

"Its mode of application is as follows: The patient is laid on his back, with his head close to a column or pivot, around which the bed is made to revolve with con-

siderable velocity. A centrifugal force is thus generated, which, from the position of the patient, determines the blood from the head towards the lower extremities. This centrifugal force, thus suddenly created, powerfully diminishes the momentum of the blood moving into the cranium: the diminished arterial pressure on the cerebral substance soon produces a sense of exhaustion or complete syncope."

The cerebral disturbances which in many cases appear suddenly after ligation or temporary compression of one or both carotid arteries, are, in the opinion of the author, likewise due to diminished vascular pressure. The more remote effects in some of these cases, viz: hemiplegia of the side opposite to the obstructed artery, and disorganization of that part of the brain supplied by the artery, arise partly from the insufficient supply of blood to the disorganized hemisphere of the cerebrum, and partly from the compression of the exsanguined hemisphere by its fellow, the vessels of which still continue to be liberally supplied with blood.

"In healthy states of the circulation within the cranium the forces distending the bloodvessels in either cerebral hemisphere are equal, opposite and counterbalance each other; but so soon as the free supply of blood to one cerebral hemisphere is cut off by the ligature on the common carotid, the vascular distension in the other hemisphere becomes a source of pressure on the exsanguined side. Hence, probably, the cause of the commencing hemiplegia, which gradually increases with the disorganization of the cerebral substance. I am inclined to attribute the successful termination to this operation in some cases to the opportune loss of blood from a wound in the throat prior to the application of the ligature: or the same happy result may be ascribed to a cautious preparatory venesection before the common carotid has been secured. By such loss of blood the circulation has been quieted, and the difference in the momentum of the blood in different parts of the brain has been, if not obviated, at any rate very much diminished. It might be objected that the effects upon tying one of the carotid arteries in the human subject are too diversified to admit of the foregoing explanation; but, in reply to such objection, it may be urged, that, although certain effects are usually produced by large abstrac-

tion of blood, still the results of bloodletting are very dissimilar in different individuals."

Dr. Burrows thinks that with proper precautions the operation of tying the carotid might be resorted to in hopeless cases of epilepsy, and some kindred maladies.

In the next section Dr. Burrows applies the opinions that he has been advocating to the explanation of the pathology of apoplexy. The most discordant opinions have been expressed as to the manner in which the apoplectic fit is produced. Abercrombie, who has done so much in cerebral diseases, recognizes the fact that apoplexy may prove fatal without any morbid appearance in the brain, and admits that he cannot explain the condition producing the fatal coma in such cases.

Coma consists in a suspension of the functions of the brain while those of the spinal cord remain more or less intact: and most inquirers in explaining it have looked too exclusively to one single cause, mechanical pressure, in the absence of which they have been unable to furnish an explanation. The causes producing it may be thus enumerated: first, pressure on the nervous fibres; secondly, division of the nervous substance; thirdly, disorganization of the nervous matter; fourthly, interrupted supply, or deficient momentum of blood in the nervous substance; and fifthly, the action of narcotics. These are all causes which, brought to act on the cerebro-spinal nerves, impair or annihilate their functions: and when acting on the brain, will produce more or less complete abrogation of its functions according as they are more or less extensively applied. Mechanical pressure, it is thus seen, is not the only source of coma; and in fatal cases of apoplexy, where a physical cause of pressure cannot be found, the presence or previous operation of some other of the causes just enumerated may be detected.

In all cases of simple apoplexy our author thinks there must have been a previous state of congestion of the vessels of the cranium, brought on either by determination of blood

to the head, or detention of blood in that part. Then a tightening of the full vessels occasion extraordinary pressure on the nervous pulp, and hence the coma. The absence of congestion after death he would explain in most cases by reference to the active treatment instituted, especially the large and repeated bloodlettings, and active purgation. Thus Abercrombie relates three cases, two of which presented no lesion of the brain beyond slight turgescence of the cerebral surface in one; whilst in the third, the membranes and substance of the brain were turgid throughout: the two former had been treated very actively by venesections, purgatives, cold to the head, etc., whilst the latter had been subjected to no such depleting treatment.

Yet in these cases of simple apoplexy, frequently the coma persists, and death ensues, notwithstanding the supposed cause of pressure is removed by bloodletting and other remedies. This is probably owing to the imperfect respiration, through which the brain becomes saturated with undecarbonized blood. The patient's condition is similar to that of one laboring under laryngitis; where if the respiration has been greatly obstructed for some hours, and the face and lips have become livid, life can seldom be preserved by tracheotomy. For a similar reason, persons who have been resuscitated after hanging and drowning gradually sink, although the circulatory and respiratory functions have been reëstablished.

Likewise in serous apoplexy, the cerebral pressure producing fatal coma is to be ascribed to the vascular distension, and not to the serous effusion. The presence of serum in the brain renders this organ more liable to suffer from congestion of its bloodvessels, and consequent vascular pressure, but is itself not to be regarded as the cause of apoplectic coma. It is but an evidence of the previous congestion; in other words, the so-called serous apoplexy is simple apoplexy, that is cerebral congestion, which has terminated by effusion.

A similar explanation is offered in sanguineous apoplexy: the coma is produced by the vascular distension, whilst the paralysis is dependent on the effusion of blood. Unless

blood is effused towards the medulla oblongata, so as to interrupt the continuity of the nervous matter between the brain and spinal cord, something more seems to be required to produce fatal apoplectic coma. Large extravasations evidently of long standing, are found in patients who have presented coma for a short time only, or not at all; and again, small and evidently recent extravasations are found in cases presenting well marked coma. If the pressure producing apoplectic coma is occasioned by the effused blood, we have the paradox of a large effusion and no coma, and a small effusion with fatal coma.

"When extravasation of blood within the cranium takes place very slowly, as from the rupture of the diseased coats of an artery, independent of determination of blood to the brain, or general congestion of its vessels, it is highly probable that the effusion does not produce the symptoms of apoplectic coma, but gives rise to some modification of hemiplegia, the paralysis being to a greater or less extent, according to the situation and quantity of the effusion. As long as extravasation of blood is actually going on, while the blood is pouring forth, it most probably produces pressure on the surrounding cerebral substance, just as the blood escaping from a divided artery in any other part of the body would compress any obstacle with a force equal to the momentum with which the blood was circulating in that vessel. When cerebral hemorrhage has stopped, I suspect the blood ceases to be a real source of general pressure, although, as it increases the quantity of extra-vascular matter in the cranium, it also offers additional resistance to the entrance of the normal quantity of blood into that cavity; hence healthy vascular distension becomes excessive, and the symptoms of general cerebral pressure are easily developed. Thus, the compression produced by extravasation will depend more on the rapidity and situation of the effusion than on the amount. If the effusion be slow, and not near to the base, although the amount be considerable, the effects will be slight in comparison.

"I therefore infer, that in the larger number of cases of apoplexy accompanied with extravasation of blood, the coma is to be attributed to antecedent or coexisting cerebral congestion; while the paralysis, which is more durable, is dependent upon the limited local mischief produced by the extravasation. The close affinity between apoplexy succeeded

by hemiplegia, and sudden hemiplegia unpreceded by apoplexy, has long been recognised. Each of these affections is usually connected with extravasation of blood within the cranium. In the former case, the hemorrhage is associated with vascular congestion; in the latter, the congestion is probably not present."

The state of congestion in sanguineous apoplexy is often not discovered after death, because, as in the other forms of the disease, the bloodvessels have been emptied by active depletion, which could have no effect on the extravasation. Perhaps, too, the congested vessels may have been partially emptied by loss of the blood constituting the extravasation.

We come now to the second part of the work, on the connection between affections of the brain and diseases of the heart. Dr. Burrows thinks that notwithstanding the attention bestowed on diseases of the heart during the last twenty years, their full agency in disturbing the functions of the brain, and producing structural changes in that organ, has not been appreciated by authors, and still less by the mass of the profession.

If it be admitted that vascular congestions, extravasations of blood, and effusions of serum, are the lesions which give rise to the chief symptoms in apoplexy and hemiplegia, it might be expected that in these affections of the encephalon some causes known to produce congestions and effusions in other parts of the body, would be found in operation.

Now of all the causes of serous or hemorrhagic effusion, diseases of the heart are by far the most common. And so the author finds, on analysing one hundred and thirty-two cases of apoplexy and sudden hemiplegia, collected from various authors, that the heart was diseased in eighty-four, or in about three-fifths: a proportion, he says, far greater than is commonly supposed, even by those who admit the occasional influence of cardiac disease in the production of the diseases named.

Of the different lesions of the heart—hypertrophy, dilata-

tion, and valvular disease—the latter is found to be the most frequent cause of apoplexy, hemiplegia, and some other cerebral affections, as it likewise is of congestions, hemorrhages, and dropsies in other parts of the body. In thirty-eight cases of apoplexy, presenting disease of the heart, hypertrophy with valvular disease occurred in nineteen, valvular disease alone in eight, and simple hypertrophy in ten.

Simply hypertrophy of the left ventricle may continue a long time before it produces serious lesion of the brain. But at length the cerebral arteries become dilated, the elasticity of their coats is destroyed, a state of constant congestion ensues, and a sudden accidental increase of vascular pressure may cause apoplectic coma. Cerebral hemorrhage may also occur if the arterial coats are diseased, as they often are in the hypertrophy of advanced life.

The influence of valvular disease is much more readily brought about.

"Valvular diseases of the left side of the heart cause, in regular succession, congestion of the pulmonary vessels, of the right cavities of the heart, of the internal jugular and vertebral veins, of the sinuses of the dura mater, and of the whole venous system within the cranium; and if, in this case, the muscular substance of the left ventricle retain its normal thickness and power, and continue to propel the blood with its wonted energy into the cranium, the substance of the brain will then, in all its parts, be placed between the action of two opposing forces—one obstructing the return of the blood from the brain, the other continually pouring fresh waves of blood into the cerebral vessels. Under this two-fold force, simultaneously distending the vessels, both veins and arteries, and causing pressure on the surrounding substance, there is no wonder that apoplectic coma should so frequently occur; or that the coats of cerebral arteries already diseased, in advanced periods of life, should yield, or be ruptured, giving rise to effusions of different kinds."

If now there be also hypertrophy of the left ventricle, all the effects just enumerated will be increased in proportion to that hypertrophy; and should there be disease of the cerebral

arteries, effusion, either serous or sanguineous, must inevitably occur.

A question closely connected with the preceding is, the period of life most prone to apoplexy and hemiplegia. The author furnishes tables of some hundreds of cases showing the number in each decennial period from twenty to eighty years, and the relative proportion in each of these periods, compared with the population of the same age. From these tables it appears, 1, "that the relative frequency of apoplexy steadily increases from 20 to 80 years," and 2, "that the actual number of apoplectic cases increases in each successive period upwards from 20 to 70 years of age, while the numbers living gradually diminish."

Now from the researches of Clendinning, we learn that the average weight of the heart increases as life advances, hypertrophy being thus a change concomitant with the period when apoplexy is most prevalent; and further, that in diseases of the heart the weight of almost all the viscera of the body, the brain among the rest, is increased. Thus in adults free from cardiac disease, the weight of the whole brain was found to be 50·5 ounces; while in those affected with disease of the heart, it was found to be 52·5 ounces.

"Hence, then, the frequency of apoplexy in advanced periods of life, the usual co-existence of hypertrophy of the heart at the same period, and the ascertained tendency of cardiac disease to produce hypertrophy of the brain, are facts which, when taken in conjunction, confirm the conclusion arrived at in the previous part of the section, namely, that the coincidence between apoplexy and diseases of the heart is something more than accidental.

"If diseases of the heart have a tendency to induce hypertrophy of the brain, it is easy to understand the frequency of apoplexy in such individuals. For if cerebral vascular pressure be the common cause of apoplectic coma, then a moderate transitory congestion, or sudden afflux of blood in an already too voluminous brain, may readily produce an apoplectic seizure. It is likewise very obvious that to such persons, if advanced in years, when the cerebral arteries are so fre-

quently diseased, a very moderate vascular congestion may be followed by cerebral hemorrhage."

In the next section, on the treatment of apoplexy and hemiplegia, Dr. Burrows, in common with many of the present day, reprobates the indiscriminate employment of the lancet; and, in addition to the usual signs indicating or contra-indicating the abstraction of blood during the "fit," insists that a special attention should be paid to those derived from an examination of the heart's sounds.

"If no cardiac disease be discovered, or if simple hypertrophy without notable valvular disease be detected, depletion, both general and local, may be carried on so far as the cerebral symptoms appear to call for that kind of relief.

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"But suppose a careful examination of the apoplectic or hemiplegic patient's heart discloses the existence of valvular disease to the extent of obstructing the circulation through its cavities, here the pulse will be a most deceptive guide as to the propriety or impropriety of abstraction of blood. If the mitral valve be principally implicated, and allow of regurgitation from the left ventricle, the small and irregular pulse so commonly observed with that lesion would probably dissuade from that free abstraction of blood which the cerebral symptoms might require. If, in another case of apoplexy or hemiplegia, the aortic valves be found diseased to the extent of not only obstructing the onward current of blood, but also of allowing regurgitation into the ventricles during its diastole, there will probably be associated with this lesion considerable hypertrophy of the left ventricle. Here will be observed a full and vibrating or thrilling pulse, but a pulse of increased action without real power, and hence a deceptive pulse; and one which, if it be regarded without reference to the structural changes of the heart, would invite to a more copious abstraction of blood than was called for by the general symptoms. In each of these last-mentioned cases greater relief to the symptoms will be obtained by a free local abstraction of blood from the vicinity of the heart (either by cupping from beneath the left mamma, or between the left scapula and spine) than by a much larger depletion by venesection.

"Again, there are other instances of apoplexy and hemiplegia, where, from an examination of the heart by auscultation,

we feel assured there is serious valvular disease; and, from the character of the cardiac murmurs and other physical signs, we arrive at a well-grounded suspicion that there is osseous deposit about the valves of the left ventricle, in the coats of the ascending aorta, and, in all probability, in the tunics of the arteries within the cranium. In such a condition of the arterial system an accidental cerebral congestion may have been followed by extravasation of blood, and thus have arisen the most common symptoms of apoplexy. The knowledge obtained through auscultation in such cases would properly dissuade from large losses of blood, although the fulness and hardness of the radial pulse might at first have invited such depletion.

"There are other cases of apoplexy and hemiplegia, where we discover dilatation of the cavities of the heart and extensive emphysema of the lungs; the latter lesion is, indeed, a more common cause of cerebral congestions and hemiplegia among the laboring classes than is commonly supposed. In such cases the appearances of extreme congestion and dyspnœa might tempt to practise large depletion, and thus the very conditions of the heart which have induced the cerebral congestions and apoplectic symptoms would be aggravated. I should here suggest the employment of the cupping-glasses to the nape of the neck, or between the scapulæ, with the internal administration of stimulating diuretics, diffusible stimulants, and the application of rubefacients to the sternum. Many mistakes arising from the causes I have now adverted to may certainly be obviated if a careful examination of the heart and lungs be made with the stethoscope in the first or congestive stage of apoplexy, or immediately after an attack of hemiplegia."

The stage of excitement in apoplexy after coma has passed away, or the symptoms indicative of inflammatory action around the clot of effused blood,—such as flushed face, hot scalp, frowning or knitting of the brows, slight strabismus, pain, usually referred to the temporal, parietal, or occipital region opposite the paralysed limb, and sometimes indicated by movements of the sound arm, or a drawing up of the palsied one,—are to be controlled by small local depletions from the temple or mastoid process on the side opposite to the paralysis, by cold to the head, by purgatives, by restric-

ted diet, and by extreme quiet in the sick-room; by a blister near the occiput when the heat of the head is diminished, and by mercury given until the gums are slightly tender, if the patient is not very old, nor extremely exhausted by depletion.

Along with these symptoms the paralyzed limbs are affected with involuntary movements—spasmodic contraction of the tonic or clonic kind—and also become the seat of severe neuralgic pains. These are not confined to the integuments but invade the deeper seated structures, and are a source of great suffering to the patient. They are accompanied with a sensation of burning heat, and with redness and swelling of the skin, and even the periosteum is found red and swollen. Sometimes they seem to depend on the continuance of irritation from the clot; at others on returning functions in the nerves, and the deranged capillary circulation consequent on the injury to the nervous system. The author has tried numerous local remedies for the removal or mitigation of these pains, but with little or no effect. When there were symptoms of remaining cerebral irritation, local depletion from the head was found serviceable, combined with evaporating lotions, tepid or cold, to the affected limbs: when these symptoms were absent, leeches to the limbs, with evaporating lotions or poultices, afforded marked relief. These pains gradually disappear after the lapse of weeks or months, and are followed by shriveling or wasting of the paralyzed limb.

In the stage of paralysis, when sufficient time has elapsed for the brain to recover from the injury done to its substance, blisters, or frictions with stimulating liniments, applied to the palsied limbs, particularly in the course of the great nervous trunks, are often successful in exciting the suspended functions of the nerves. If the employment of a more direct stimulus be resolved on, electricity is the most appropriate; but it must not be used unless there is "total absence of headache and other cerebral symptoms," and unless "the patient's mind remain firm, and without alarm at the ordinary effects of the electric current." In hemiplegia no decided

benefit was derived from this remedy at the hospital to which the author is attached, though in paraplegia its good effects are often unquestionable.

The author's opinion of strychnine is still less favorable; often the remedy seemed to aggravate the harassing pains in the affected limbs. A safer and more beneficial mode of restoring the innervation, is by regular frictions over the surface of the palsied extremities, and well-devised exercise of their muscles.

The benefit to be derived from counter-irritation by seton or issue at the nape of the neck, will depend upon the cause of the cerebral congestion producing the seizure. If this cause is found in some idiopathic affection of the brain, such permanent counter-irritation may modify or suspend its action; if it consist in some of the serious lesions of the heart already alluded to, counter-irritation at such a distance from the affected organ will be useless, and sometimes injurious.

In the sixth section Dr. Burrows points out some head affections which are often dependent upon or seriously aggravated by cardiac disease. Severe attacks of *epistaxis* occurring in middle-aged persons, who have not habitually suffered from that discharge, may often be traced to structural changes in the heart, either hypertrophy or valvular obstruction, especially disease of the mitral and aortic valves. This hemorrhage may, he concludes, often be considered as strictly pathognomonic of an obstructed circulation through the heart, as hemoptysis is symptomatic of tuberculated lungs, or intestinal hemorrhage of an indurated liver. Obscure headaches, accompanied with vertigo, some impediment of speech, and muscular tremors; a great variety of symptoms occurring in persons supposed to be dyspeptic, hysterical, nervous, or on the verge of insanity; insanity itself; delirium tremens; are often found complicated with heart disease. The suggestion is a little startling, that, if auscultation were generally practiced in hospitals for the insane, numbers would be found suffering from cardiac lesions, which, by disturbing the

circulation, tend to keep up the disordered functions of the brain.

The last section is on the affections of the brain and spinal cord depending on acute diseases of the heart. The author cites cases of inflammation of the brain and its membranes, of mania, dementia, apoplexy, epilepsy, tetanus, trismus, chorea, hysteria—of nearly all forms of cerebral irritation—which were simulated by carditis and pericarditis.

But here we must leave our author. Our notice is somewhat lengthy, more so than we intended; but nevertheless we have been compelled to pass more hurriedly over some chapters than we desired. We have perused the volume with great interest, although something of it was already familiar to us; and we recommend all our readers to consult it.

Dr. Burrows' experiments testing the correctness of Kellie's conclusions are not the first that have been instituted for that purpose. The Western Journal of the Medical and Physical Sciences, vol. ix., contains the details of some performed on dogs and rabbits by Dr. Gross, who was thus led to dissent from the opinions advocated by Kellie. It was our design to notice these experiments more particularly, in connection with those of our author, but want of space prevented it. A short account of them may be found in Gross's Elements of Pathological Anatomy. C.

# SELECTIONS

FROM

# AMERICAN AND FOREIGN JOURNALS.

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*On the present state of our Knowledge with respect to the Uses and Effects of Chloroform.*—On the 3d ult., Dr. Nevins read a paper on this subject before the Liverpool Medical Society, in which he informed them that he had received private communications up to that date from several of the London hospitals, from Edinburgh, the Liverpool hospitals, and from Leeds and Birmingham, the York Lunatic Asylum and Retreat, the Wakefield Asylum, Dr. Conolly, Dr. Churchill, and other private sources, beside the cases recorded in the public journals; and, from the candid tone in the letters, and the particulars contained in them, he thought the Society might consider itself in possession of the unfavorable effects and results of this agent, as well as its favorable ones. He should, however, chiefly dwell upon the former, as they were the most important at present towards obtaining a full knowledge of its properties; the reports in the journals having fully proved, that it may be advantageously administered in many instances, and having confirmed the utility of its employment in most of the cases in which ether has hitherto been used.

*Vomiting.*—Amongst the immediate effects produced by its administration, vomiting was frequently present. It often seemed dependent upon the food having been recently taken; but in several instances neither food nor drink had been swallowed for several hours previously. It was, however, very important to attend to this particular, as in several cases chloroform had produced no effect whatever when

inhaled even in large quantities immediately after a full meal.

*Headache* was another not unfrequent result, though the contrary had often been stated. In some cases this continued for several hours, and in one instance it had not entirely disappeared for some days. It was principally, if not entirely, confined to the forehead.

*Convulsions* were very frequent, and he used the term "very" advisedly, because they had been often reported to him. In one or two reports it was said, "Nearly all the patients operated upon, were more or less convulsed;" and in some instances this had rendered the operation almost impracticable. In a case of tying the brachial artery the muscles of the arm were so violently affected, that the vessel could scarcely be gained, and the accompanying veins were so swollen and turgid as to obscure it even when exposed—rendering the operation very unsatisfactory. In another case—an operation upon the perineum—the urethra and other parts were so forcibly and suddenly retracted several times, as to remove them from the control of the operator; and in a third case,—one of lithotomy—it required the vigorous efforts of four persons to hold the patient on the table even after being tied up in the proper position. Several other cases had been mentioned; and, though he could not give an accurate statistical report, he thought about one patient in six or eight was affected with them more or less severly. In many cases they were accompanied with frothing at the mouth, whilst in others they resembled common hysterical symptoms. One important caution to be learnt from this was, never to attempt any operation more severe than tooth-drawing without having an assistant at hand. He had lately been prevented from completing an operation satisfactorily, by the patient starting up in the middle of it, in a state of excitement almost like that produced by laughing gas, and, before he could be replaced on the sofa, the effects of the chloroform had quite gone off, and the operation was finished whilst the patient was conscious. He was not aware that he had started up or exhibited any degree of violence. It was sometimes supposed that convulsions only occurred before the full effect of the chloroform had been produced; but in many of the reports it was distinctly specified that "the patient was fully under its influence," and this had been the case in his own instance. It was fortunate, however, that these convulsions scarcely ever commenced *during* the operation; they were always manifested as soon as the agent began to

take effect, if they were present at all; and therefore a surgeon was not liable to be betrayed into commencing an operation during their absence, and being interrupted by their subsequent appearance. If they were so severe as to interfere with the operation, he had nothing to do but wait till the effects of the chloroform had gone off, and then his patient was in no less favorable a state than if he had simply not known of or used that agent; and he might remark, that not a single instance had been reported to him, or publicly recorded, in which any permanently injurious effects had resulted from these convulsions. Except as a matter of inconvenience, the clenching of the teeth often observed prior to tooth-drawing did not deserve particular notice.

*Depression or Prostration.*—This was generally produced, more or less, and sometimes to an alarming degree, though hitherto no fatal issue had been traced to the employment of chloroform. It generally diminished the number of pulsations, and in one or two instances both the heart and lungs had almost ceased to act, and fears were entertained that the patients would die under the operation, but they had rallied, and had all done well ultimately. In most cases the reports stated that re-action had been perfect in from ten minutes to two hours; and, so far as our present knowledge extended, the depression produced was not of an alarming or dangerous nature. In one or two cases, indeed, in which the patients were reported to have been at death's door, and pulseless, from uterine hæmorrhage, or prostration otherwise induced, they rallied under the influence of the chloroform, and the operations were safely performed though ordinary stimulants had been previously tried in vain.

*Secondary Hæmorrhage.*—One of the reports says that the writer fancies there has been a greater disposition to secondary hæmorrhage, than in cases in which chloroform has not been used; but he is the only observer who has noticed this. The reports generally made are, "Very trifling hæmorrhage," "No secondary hæmorrhage," &c., and in the midwifery cases this was very striking. In several instances in which there was previously a disposition to flooding, there was none when chloroform was used; or without any previous flooding of a serious character, the reports state, "Very little discharge," or "Less hæmorrhage than usual." The result of experience thus far is therefore in favor of this agent, as regards this particular effect.

*Excoriation* of the lips and nose had generally been stated to be caused by using an impure specimen; but it had also been

observed in so many cases in which every care had been taken to have pure chloroform, that it must be considered as an effect of the agent itself. It may, however, be prevented by taking care to avoid direct contact.

*Pathological appearances after death from Chloroform.*—The recent experiments by Mr. Wakley, in addition to those of Dr. Gibson, of Newcastle, Mr. Gore, Mr. Gruby, and others, have proved indisputably that death may be caused by the inhalation of chloroform vapour, or by its introduction by injection into the vascular system. In all these cases the post-mortem appearances were the same:—"Excessive congestion of the lungs and large vessels of the heart, such as was perhaps scarcely ever witnessed in post mortem examinations."—Wakley. "Enormous congestion of the lungs, so that they appeared almost like one vast apoplectic spot,"—Gibson. "Great congestion of the lungs—not very great congestion of the vessels of the brain."—Gore.

Several cases of death at periods varying from twenty-four hours to two or three days, were mentioned in the reports from which Dr. Nevins derived his information; but unfortunately they were nearly all cases of operation for hernia, or upon the abdomen in some way, and peritoneal symptoms had been chiefly sought for, while the state of the brain was not once alluded to, and that of the lungs very slightly in only one or two, and in these no mention was made of great congestion. In one respect the experiments of Mr. Gore were particularly interesting in their bearing upon midwifery. He killed a rabbit which was nearly at the full period of utero-gestation, by the repeated inhalation of chloroform vapor, and then extracted six young ones from the uterus of the mother, which all lived for several minutes. Dr. Nevins had been struck by the few cases of still-born children in the midwifery reports sent to him. He had the particulars of about eighty cases of labor in which chloroform or ether was administered for periods varying from ten minutes to sixteen hours and a half, of which eighteen were cases requiring turning or instrumental assistance. Six children only were still-born; of these, two had undergone craniotomy; one was a funis presentation; one was turned for placenta prævia; and the other two were restored by appropriate treatment. In fact it appeared as if the child had a better chance of life after the employment of chloroform than without it, as it was usual to have a greater number of still-born children with such cases as had been reported.

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From the experiments of Mr. Gruby, it appeared that the

uninterrupted inhalation of chloroform for from three to five minutes caused death in several of the animals experimented upon, whilst similar animals breathed the vapor for upwards of an hour and a half, without injury, if occasional draughts of unmixed atmospheric air were interposed; from which the important inference might be drawn, that we ought, in every case of its administration, to remove the sponge occasionally, and allow the patient to inspire pure air alone.

The statement that the vitality of the blood was impaired by anæsthetic agents was not borne out by facts; for in Dr. Snow's report of ether cases he says the blood coagulated firmly in every case, and the jets of blood from divided arteries had the usual vermilion color. This was observed in many of the reported cases, whilst in one or two the color was said to be "perhaps not quite as light as usual, but the venous blood was not so dark;" and in a rabbit killed by chloroform, Mr. Gore found that the blood drawn from the jugular and crural veins just before death coagulated quickly and firmly. The inference drawn by Mr. Nunn, of Colchester, from the fluid state of his patient's blood after death, was, therefore, not confirmed by other cases.

*Midwifery.*—Dr. Nevins had the reports of about eighty cases of labor under the influence of this agent, the general result of which was highly favorable. No case of death has reached him in which this termination could be attributed with any fairness to it. One woman had died of puerperal fever on the third day after its employment, and a second patient had also died about the same date with sloughing of the os uteri and interior of the uterus, after the application of the short forceps; but he had ascertained that at the same time one or two cases of puerperal fever had occurred in the practice of surgeons who had used chloroform, and erysipelas was also prevalent in the town at the same time. Now it has been often observed, that when the latter disease is prevalent, cases of puerperal fever were also met with; and there was, therefore, no presumption that the occurrence of the fever had any connection with the employment of the chloroform. Another patient had been affected with œdema of the epiglottis, commencing about twenty-four hours after labor, in which she had cried out a good deal prior to the exhibition of the chloroform, but not so much as many women in ordinary labor. Here, then, a presumption might arise that the œdema was owing to the inhalation, but further experience was necessary to confirm or disprove it. Another patient began to rave violently after the inhalation, but she had been previously much

excited by the number of persons around her, (students and others,) who had questioned her frequently while preparing for its administration, and when just beginning to be under its influence: she ultimately did well. Another young woman with her first child, to whom ether was given, which was afterwards ascertained not to have been of the full strength, had severe convulsions beginning about ten hours after labor was completed. By bleeding and the common treatment, she was cured in the ordinary length of time.

With these exceptions, no untoward circumstances had occurred after the use of the chloroform; and it was evident that any connection, except that of time, between some of these and its employment, was very doubtful.

The general description of the labors was, that the patients accomplished them in the usual time, but without the fatigue of ordinary parturition, and they were entirely free from the exhaustion so commonly experienced afterwards: they expressed themselves as if the labor had scarcely been of any consequence, and the recoveries, with the above exceptions, were all described as "unusually quick and favorable." In many, perhaps most cases, the after pains were decidedly less than usual, or than they had been in previous labors. In several, however, they were as severe as usual, in none more so. If administered prior to the dilatation of the os uteri, no particular effect was noticed upon it; but when the vagina was hot, dry, and swollen, as in some of the cases, it generally became soft and moist almost immediately. In about one-third of the cases, the uterine contractions were decidedly enfeebled, and the intervals lengthened; but so much relaxation of the soft passages was produced at the same time, that, with one or two exceptions, the labor was not reported as having been prolonged beyond what might have been anticipated had chloroform not been used. In nearly every instance, the abdominal muscles acted in concert with each uterine contraction, and the legs were generally stretched out at the same time, and the patients often uttered a low moan during each pain, but without being subsequently conscious of having suffered. In many cases they were aware of the passage of the child's head over the perinæum, but it was unaccompanied with pain. The placenta was expelled as usual, and there was less hæmorrhage than common in most of the reports; our experience, therefore, is so far decidedly in favor of the safety and utility of its employment. In one case it was continued at intervals for sixteen hours and a half, and in

others for varying periods down to ten minutes, just during the passage of the head over the perinæum.

*Capital Operations.*—So many favorable cases have been reported, as to have established the general safety and utility of its employment, and Dr. Nevins confined his observations, therefore, to the unfavorable ones, of which about twenty had been reported, with particulars. He could not, however, imagine that the chloroform could have had any influence in such cases as the following, which described many of the unfavorable ones:—"Severe compound fracture—amputation—secondary hæmorrhage, or erysipelas—death in a week." Or "Age 70—state prior to operation unfavorable—death in a week from sinking." Of the whole number there was not one in which the fatal result was unequivocally, or even very probably, owing to the chloroform; but it did so happen that three out of four cases of strangulated hernia, sank without anything particular to make the operator anticipate it beforehand; two of the patients were upwards of sixty-five years old, but this was the only circumstance apparently against them. In the third case the patient was under the operation nearly three-quarters of an hour, and the gut was in a bad state at the end of that time.

Erysipelas was reported to have occurred in several cases, but it would be impossible to trace any connection between this and the use of chloroform from our present experience. It is not probable that they have any relation of cause and effect.

The other untoward cases have been already mentioned in speaking of the immediate effects produced by chloroform.

*Delirium Tremens.*—Four cases only have been reported by as many different authorities, in all of which chloroform produced the best results: the patients immediately fall into a tranquil sleep, from which they awoke in from one to four hours. In some of the cases opium was then given, in others not; but in all, the patients again fell asleep for several hours, and awoke free from the symptoms of the disease.

*Mania.*—In this disease experience has not hitherto been so favorable. Two authorities mention having used it with benefit in two cases of high maniacal excitement. In the other cases reported (about a dozen) no permanent benefit, and scarcely even temporary relief, was obtained.

*Tetanus.*—In one case the patient recovered; in another, slight temporary benefit, and ultimately death; in a third, immediate aggravation of the spasm was produced.

*Chorea.*—Only one case reported: in this it did no good.

*Spasmodic Asthma.*—In two cases reported it produced a most beneficial effect; though in one of them ether had aggravated the distress in a previous attack.

*Tic Doloreux.*—Two cases reported; in these chloroform was used with immediate temporary relief, followed by sleep, and no return after an interval of many days; relief was also obtained in a distressing case of hysterical convulsions, with great spinal irritability.

Mr. Ellison read a report of the fatal administration of chloroform in Newcastle, which had appeared in the Times of that morning. In this case death ensued in three minutes in a healthy girl, who had previously inhaled ether without inconvenience. There was extreme congestion of the lungs, and considerable congestion of the brain.

Mr. Kay had tried chloroform in one case of midwifery requiring the application of the short forceps. The patient's convulsive movements were so violent, that he was obliged to wait until the effects of the chloroform had gone off, before he could apply them.

Dr. Roche had met with a similar case, and he thought the recent fatal case might not be without benefit, for he had been called to attend a young lady during her first confinement, and found her busy inhaling chloroform on his arrival, of which she had obtained a supply spontaneously, and which she had been practising upon for a day or two to see how it answered. If patients began to take it into their own hands in this way, we must expect to hear of more fatal consequences.

Dr. Nottingham had assisted at an amputation of the thigh, in which it was difficult to distinguish between the color of the arterial jet of blood, and the venous oozing. The patient, however, did well. He had observed this also in another case.

Mr. Higginson inquired whether the author of the paper had observed the vibratile condition of the pupil before the patient became completely under the influence of the chloroform. He had noticed this in two or three instances, and that the pupil became widely dilated when under its full influence. He also wished to know the earliest age at which this agent had been administered with safety.

Dr. Roche had operated satisfactorily for nævus upon a child not three months old, to whom he had given fifteen drops of chloroform; it continued under its influence six minutes.

Dr. Nevins had not noticed the condition of the pupil referred to. He had seen a report of a successful operation upon an infant six months old.—*London Medical Gazette.*

# THE WESTERN JOURNAL.

THIRD SERIES.—VOL. II.—NO. II.

LOUISVILLE, AUGUST 1, 1848.

## ASSOCIATION OF MEDICAL SUPERINTENDENTS OF AMERICAN INSTITUTIONS FOR THE INSANE.

We have received from Dr. Kirkbride, secretary of the Association, the proceedings of the Superintendents of the Institutions for the Insane, at their meeting in New York in May last, and in the belief that they will prove interesting to our readers, we make copious extracts from them.

The Association of Medical Superintendents of American Institutions for the Insane commenced its third annual meeting at the Astor House in the city of New York, on the 8th of May, 1848, the vice president, William M. Awl, M.D., in the chair, and Thomas S. Kirkbride, M.D., secretary.

Present—Dr. James Bates, of the Marine Insane Hospital at Augusta; Dr. Andrew McFarland, of the New Hampshire State Hospital at Concord; Dr. Wm. H. Rockwell, of the Vermont State Hospital at Brattleboro; Dr. Luther V. Bell, of the McLean Asylum for the Insane at Somerville, Mass.; Dr. C. H. Stedman, of the Boston Lunatic Asylum; Dr. N. Cutter, of the Private Institution at Pepperill, Mass.; Dr. John S. Butler, of the Connecticut Retreat at Hartford; Dr. Amariah Brigham, of the State Lunatic Asylum at Utica, N. Y.; Dr. Pliny Earle, of the Bloomingdale Asylum, N. Y.; Dr. James Macdonald, of the Private Institution at Flushing, L. I.; Dr. —— Renney, of the Lu-

natic Asylum on Blackwell's Island, N. Y.; Dr. G. H. White, of the Hudson (private) Lunatic Asylum, N. Y.; Dr. Horace A. Buttolph, of the New Jersey Lunatic Asylum at Trenton; Dr. Thomas S. Kirkbride, of the Pennsylvania Hospital for the Insane at Philadelphia; Dr. Joshua H. Worthington, of the Friends' Asylum at Frankford, Pa.; Dr. N. C. Benedict, of the Blockley Insane Asylum at Philadelphia; Dr. Fonerden, of Maryland Hospital at Baltimore; Dr. Wm. M. Awl, of the Ohio Lunatic Asylum at Columbus; Dr. John M. Galt, of the Eastern Asylum of Virginia at Williamsburg; and, Dr. John R. Allan, of the Kentucky Lunatic Asylum at Lexington.

Dr. Samuel B. Woodward tendered his resignation of the Presidency of the Association, which was accepted, and Dr. Wm. M. Awl was elected President in the place of Dr. Woodward, resigned; and Dr. A. Brigham, Vice President, in the place of Dr. Awl, elected President. The following preamble and resolutions in reference to its late President, were unanimously adopted by the Association, viz:

Whereas, Dr. Samuel B. Woodward, at the present meeting of this Association, has tendered his resignation as President thereof,

Resolved, That whilst accepting this reignation, we cannot adjourn without declaring our high sense of the services of Dr. Woodward as President of this body, and also our full appreciation of his ardent and useful exertions for so many years in behalf of the unfortunate insane.

Resolved, That the Secretary of the Association be requested to transmit to Dr. Woodward a copy of this resolution.

Agreeably to appointment, Dr. Brigham read an obituary notice of the late Dr. White, of the Hudson Lunatic Asylum and the first Vice President of this Association, which was directed to be entered upon the minutes.

Dr. A. V. Williams and Dr. Benjamin Ogden, two of the visiting physicians of the Asylum on Blackwell's Island, were invited to attend the sittings of the Association; and a resolution was adopted, authorizing each member to invite any person interested in its discussions.

In conformity with a resolution, adopted at the last meeting of the Association, Drs. Brigham and McDonald made written, and Drs. Earle, Rockwell, Bates, Butler, Allen, and Kirkbride, verbal reports on the subjects of post-mortem examinations and the pathology of insanity, which, after consideration, were referred to the standing committee on these subjects.

Dr. Kirkbride read a report, from the committee on publication, which was accepted, and the Association subsequently resolved, That the committee on publication, appointed at the last meeting, be continued, and instructed to publish such of the reports, and such parts of the reports made to this Assiciation, and such parts of its proceedings as they shall deem conducive to the public good.

Written reports were made on the following subjects, and, after full discussion accepted, and laid upon the table, subject to future disposition by the Association, viz:—

On the comparative value of the different kinds of labor for patients, and the best means of employment in winter, by Dr. Rockwell.

On the advantages and disadvantages of cottages for wealthy patients, adjacent to hospitals for the insane, by Dr. Kirkbride.

On the relative value of the different kinds of fuel, for heating hospitals, by Dr. Bates.

On the most economical mode of treating the insane of the poorer classes, by Dr. McFarland.

On reading, recreations, and amusements for the insane, by Dr. Galt.

On the comparative value of treatment in public institutions, and private practice, by Dr. White.

On the effects on the insane, of the use of tobacco, by Dr. Cutter.

Remarks on the diseases and causes of death among the insane, were also read by Dr. McDonald; on the statistics of insanity, by Dr. Earle; and a series of cases of mania-a-potu, treated by the inhalation of ether, in the Boston City Hospital, by Dr. Stedman.

The following preamble and resolution were adopted by the Association. viz:

Wheras, in the selection of medical superintendents to American institutions for the insane, it is important to choose men with the highest qualifications, both as respects professional acquirements and moral endowments: therefore,

Resolved, That any attempt, in any part of this country, to select such officers through political bias, be deprecated by this Association as a dangerous departure from that sound rule which should govern every appointing power, of seeking the best men, irrespective of every other consideration.

The following resolutions were also adopted during the different sessions of the Association:

Resolved, That a committee be appointed to report to this Association, at the next meeting, the best terms for the classifications and designations of the different forms of insanity, and also the best anatomical and pathological terms for the various parts of the brain, and a nomenclature of the diseases which prove fatal to the insane.

Resolved, That a committee be appointed to suggest the best plan of calling the attention of physicians in general practice to the proper treatment of the insane at their homes, and especially to their treatment during the first period of their disease.

Resolved, That the members of this Association be requested to predare and present to a future meeting, a statistical analysis of all the cases of insanity which have been admitted into the different institutions under their care.

Resolved, That all subjects heretofore referred to committees and not reported on at this meeting of this Association be continued in the hands of the present committees for future action.

Resolved, That a committee be appointed who shall, either before

or after our adjournment, select subjects and appoint members to report on the same, in writing, at the next meeting of the Association.

Resolved, That previous to the future meetings of the Association the secretary be requested to invite the Boards of Trustees, managers, or official visitors of each insane asylum on this continent, to attend the sessions of this body.

Resolved, That the thanks of this Association be tendered to Messrs. Coleman and Stetson, of the Astor House, for their very liberal provisions for the meetings of the Association, and for which, on account of its benevolent objects, they have declined receiving any compensation.

Resolved, That the thanks of the Association be tendered to the officers for the able manner in which they have performed the duties of their respective stations.

Resolved, That the Secretary be instructed to furnish an abstract of the proceedings of the Association to the editor of the American Journal of Insanity, and to the editors of the various Medical Journals in the United States, for publication in their respective periodicals.

The Association continued its sessions until the afternoon of the 12th of May, and then adjourned to meet in the city of Utica, N. Y., on the third Monday of May, 1849, at 10 o'clock, A. M.

By order of the Association,

THOMAS S. KIRKBRIDE, Secretary.

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### THE LATE MR. LISTON.

The property left by this distinguished surgeon amounts, it is stated, to £5,000 only, exclusive of that arising from the sale of his effects. We have seen his former yearly income from his practice set down at twice this amount.

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### WESTERN LANCET.

This Journal began its eighth volume with the number for July. It is now published monthly, each number containing sixty-four pages. Dr. Lawson has associated with him in the editorial management, Dr. J. P. Harrison, one of his colleagues in the Medical College of Ohio. Both gentlemen have abilities and zeal, and we trust that their labors will meet with fitting recompense.

### RESIGNATION OF MR. SYME.

Mr. Syme has resigned the Chair of Surgery in University College, London. He returns to Edinburgh, and will be a candidate for the chair of surgery in the University of that city. His friends state that he was successful in private practice during his brief sojourn in London, and that he resigns solely from a desire to avoid being made a party in the angry dissensions in which the council and officers of University College and Hospital are involved. He was waited on by a deputation of students, requesting him to recall his resignation, but he declined, stating that it would neither be conducive to their interests nor to his own for him to remain longer connected with the college and hospital.

As we understand it, the whole difficulty had its origin in the selection of Mr. Syme to fill the vacant chair. The profession in London, and especially the former pupils of University College, thought their claims had been disregarded in the appointment of an Edinburgh man, a Scotchman, to fill a vacant professorship of surgery in London.

Another account states that Mr. Syme assigns as his reasons for resigning, "his unwillingness to undertake the whole of the systematic course, and his deep sense of the insult offered in his presence to his friend Dr. Sharpey by the students of University College, the slightest approach to which would effectually incapacitate him from ever addressing his pupils with satisfaction to himself or benefit to them."

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### "HONORABLE FEES."

We find the following paragraphs in the Southern Medical and Surgical Journal for July:

"We were recently called into consultation with the Professor of Obstetrics, etc., in the Medical College of Georgia, to attend a lady in puerperal convulsions. When the case terminated, a bank check, signed by the husband, who is a member of the Bar, with the amount left *blank*, was sent to each of us.

"*Another.*—We learn that a surgeon in Savannah, having operated successfully on the son of a lady in that city, had, besides his usual fee, a splendid case of surgical instruments presented to him by the grateful mother. These are truly green spots in the arid professional path, and we have to regret they are so far apart."

We have quoted these partly that others may feel as much gratified as we did in perusing them, and partly as *per contra* to the notice of fees in our last number. It is something to know that Louisville is not the only place in which surgery is appreciated.

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NEW ORLEANS MEDICAL AND SURGICAL JOURNAL.

Drs. Fenner and Carpenter have withdrawn from the editorial department of this Journal, leaving Drs. Harrison and Hester. We are pleased to learn that the Editors' forebodings in a previous number have not been realized; and that the work is now, at the beginning of the fifth volume, placed beyond the reach of contingencies. We always open it with pleasure, and are glad that it is so well supported in the south and southwest.

This reduction of the number of editors seems to have had a happy effect on one of the contributors, rousing him from a kind of Rip Van Winkle sleep. The number for July contains a notice of the second edition of Gross's *Elements of Pathological Anatomy*, published in 1845! Now that the writer has discovered the existence of the book, he does not seem fully awake to its merits; he complains that it is behind the times, though he must acknowledge that it is *three years ahead of himself!* As he has not carried his examination far beyond the "preface to the first edition," perhaps increase of appetite may grow by what it feeds on during his further progress. But this depends, we may say, a good deal upon the *animus;* for the book is one of those which are

> ——"ut illius animus, qui ea possidet:
> Qui uti scit, ei bona; illi, qui non utit recte, mala."

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ISOPATHY.

The London Medical Gazette states that a new humbug has made its appearance in Germany. A Dr. Hermann has discovered that "every diseased organ has its remedy in the same organ." Thus, if one has a diseased liver, eating liver will restore him; if he suffers with headache, brain will cure it; if he has an affection of the bladder or kidney, the bladder or kidney furnishes a corrective; and if the testicles are the source of discomfort, "mountain oysters" are invaluable! As the organs in substance might not be palatable to people with weak sto-

machs, the wise Dr. Hermann uses them in the form of tincture, under the scientific names of ***stomachine, cystine, testiculine, cerebrine,*** *etc.* A book, containing fifty cases of radical cures, has already been published at Augsburg.

This announcement is made for the especial benefit of two or three men in this city who have run the round of homœopathy, hydropathy, and "medical galvanism," and to whom this new ***'pathy*** will be a perfect god-send. If we can give them any further information we shall do so cheerfully. If they desire it we will put our knowledge of German to some use by translating Dr. Hermann's book for them. There need be no delay for want of tinctures; the cabinet of pathological anatomy at the University will afford them a stock ready made.

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### FALL LECTURES.

The inducements for students of medicine to visit our city for instruction during September and October, are somewhat greater than usual. Lectures will be delivered in the Medical Department of the University on *Visceral and Surgical Anatomy*, by Prof. COBB; *Operative Surgery*, by Prof. GROSS; *Pharmaceutical Chemistry*, by Prof. YANDELL; and *Practical Obstetrics*, by Dr. BAYLESS. Dr. COLESCOTT will lecture at the Hospital on *Clinical Surgery*, and on the 16th of October will begin his second course on *Diseases of the Eye and Ear;* and Dr. DAVID W. YANDELL will give instruction in *Physical Diagnosis and Diseases of the Chest.* We trust that all these lectures will be well attended.

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### NEW BOOKS.

We have received from the publishers the following books, which will be attended to at an early day: Stille's Elements of General Pathology; Hastings on Yellow Fever; Mayne's Dispensatory and Formulary; Manuals on the Blood and Urine, by Griffith, Rees, and Markwick; Bird's Elements of Natural Philosophy (a capital book); Blackiston on Diseases of the Chest; Williams' Principles of Medicine, 3d edition; Gardner's Chemistry; Sargent's Minor Surgery; and Meigs on the Diseases of Children. These books are for sale at the store of M. A. MAXWELL.

THE

# WESTERN JOURNAL

OF

# MEDICINE AND SURGERY.

# EDITORS.

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DANIEL DRAKE, M. D.,

PROFESSOR OF PATHOLOGY AND THE PRACTICE OF MEDICINE IN THE UNIVERSITY OF LOUISVILLE.

LUNSFORD P. YANDELL, M. D.,

PROFESSOR OF CHEMISTRY AND PHARMACY IN THE SAME

THOMAS W. COLESCOTT, M. D

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